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OBSTRUCTION TO RIVER DISCHARGE BY BRIDGE PIERS.

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In 1881 the question of crossing the line of the New York, Lake Erie and Western Railroad by the line of the New York, Lackawanna and Western Railway, then under construction through the town of Chemung, Chemung Co., New York, came before a commission at Elmira, N. Y. (For the sake of brevity these roads will be designated the Erie Railroad, and the Lackawanna Railroad, respectively.)

The place selected for the crossing by the Lackawanna Company is about ten miles east of Elmira. At this point and for some miles west of it, the Erie Railroad skirts the base of the hills on the south side of the Chemung River Valley, while to the eastward its track leaves the high ground and traverses the bottom lands for a distance of about three miles, and then crosses to the north side of the Chemung River.

The entire width of the valley, including the flats on both sides, subject to overflow in times of freshet or flood, varies from one-half to three-fourths of a mile, and within this width the river, while pursuing its southeasterly course toward the Susquehanna, of which it is a tributary, meanders from side to side, in the manner usual with streams flowing through alluvial lands. The bed of the stream as defined by its banks at ordinary stages, has a width ranging from 400 to 500 feet, and the banks themselves are ten to fourteen feet in height above the ordinary low river-stage. The surface of the bottom lands is somewhat undulating and irregular, so that a flood rise of twenty feet above extreme low river overflows them to a depth varying from four to ten feet, the average depth being not more than five and a half feet. With a flood of this height, which is not of frequent occurrence, the width of the river at Baldwin's, opposite the point where the railroads cross each other, is 3900 feet. Half a mile below this point, that is to the eastward at Parshall's, the flood width is 3200 feet, and half a mile to the westward it is about 4000 feet; but on this westerly section the highway near the bank of the stream, on the north side, is not put under water by a twenty foot rise, although the bottom land between it and the high ground to the northward is submerged to the depth of six feet in some places. The sections at Parshall's and Baldwin's are given at profile A and profile B respectively, on the map.

The average of the three widths given above, or 3700 feet, $\left(\frac{3200+3900+4000}{3}\right)$ may be fairly assumed as the mean width of the stream during a twenty foot rise,

on the reach about one mile in length above Parshall's.

It is in evidence that a flood of this height occurred in March, 1865, and maintained its highest level for several hours, submerging the Erie track to the depth of the floor of a hand car, and causing an entire suspension of traffic on that part of the road.

The river at the point of crossing drains an area of about 3000 square miles, and at ordinary low stage is an insignificant stream, in some places not more than 100 feet wide, consisting of a succession of ripples and slackwater pools, the general slope, as ascertained by precise levels, being four feet to the mile. The depths in the pools vary from four to ten feet. The low river discharge probably does not exceed 3000, possibly not 2500 cubic feet per second.

The track of the Erie Railroad, in the vicinity of the crossing, and for some miles on either side of it, has an easterly descending grade varying from five feet to seven feet to the mile. Its general height above the low-river surface is from seventeen and a half to eighteen feet, and above the general level of the bottom lands about three and a half to four feet on the average.

A narrow branch of the river, or more properly speaking perhaps, a slough or cove known as Parshall's Cove, about as deep as the main stream, runs in close proximity and nearly parallel to the track of the Erie Railroad at the point of crossing, and to the east and west of it, for a distance of fully three-fourths of a mile, thus separating it from the bottom lands.

During the flood stage, when the flats are under water, this slough runs with a high velocity and discharges a comparatively large volume of water. At ordinary stages however when the river is not over its banks, the upper end of this cove has no connection with the main stream, the water in it being stagnant, and entering mostly from below, where the connection is kept up even during the low-river stage.

The Lackawanna Road approaching from the east, as projected, crosses the Erie Road above grade at an acute angle of twenty-six degrees, then crosses Parshall's Cove at the same angle on a bridge of three spans of 150 feet each, measured parallel to the track, giving an effective aggregate opening of only 180 feet at right angles to the axis of the Cove, then crosses the bottom lands diagonally with a descending grade of 13.728 feet per mile, on a solid embankment, for a distance of about 3300 feet to the right bank of the Chemung River, crossing that river at an angle of about seventy-eight degrees with the direction of the current, on a bridge of four spans, each of 150 feet clear opening, equivalent to an effective opening at right angles to the stream of 576 feet. From the abutment on the left bank, the Lackawanna Road, as projected, continues on a solid embankment to the high ground on the north side of the valley.

The line of flood level on the sections taken at Parshall's and Baldwin's, profile A and profile B, is that of the flood of 1865. Adopting 3700 feet as the average width at the flood stage of these two sections and the one-half a mile above Baldwin's, it will be seen that the Lackawanna Railroad, as projected, leaves an effective opening in their construction across the valley, from the high land on one side to the high land on the other, of only 756 feet (180 + 576), thus actually reducing the width of the high river water way nearly eighty per cent.; that is, the obstruction consisting of embankment and bridge piers and abutments, would amount to nearly four-fifths of the entire width pertaining to a flood like that of 1865.

The effective water way, however, would not be obstructed in nearly so great a proportion, because the bridge openings are located in the deepest water. By measuring the area of the three flood sections referred to above, we obtain 31,538 square feet as the mean sectional area $\left(\frac{24235 + 27294 + 43085}{3}\right)$. The flood area under the bridges on sections taken at right angles to the thread of the current is 13,068 square feet. The effective water way for a twenty-foot rise, like that of 1865, would therefore be obstructed $58\frac{6}{10}$ per cent. by the Lackawanna Road, as projected, provided we assume that the proper way to measure the clear or effective water way in oblique crossings, is at right angles to the thread of the current and the faces of the piers.

The Erie Road opposed the location

and method of construction adopted by the Lackawanna Road for crossing the Valley, on the ground that so serious an obstruction to the passage of the floods would cause an increased rise or banking-up of the water to such a degree that the destructive effects of the higher floods would be greatly increased, while those of ordinary magnitude, which would otherwise be harmless, might thereby be rendered dangerous and destructive. It was also claimed that there would be danger from ice gorges, and that the funnel shaped opening between the two tracks, above their intersection, with its base pointing up stream, rendered the accumulation of ice and the formation of an ice gorge at the Cove bridge highly probable.

During the trial before the Commissioners, professional engineers were placed upon the witness stand by both parties.

As there had been no extraordinary flood in the river since the question of crossing first arose between the two Companies, and as there had been no observations of a scientific character made of the flood of 1865, or of any flood indeed, it was found that there was not much positive knowledge available, concerning the behavior of the river in times of bank overflow, more especially its slope, mean and bottom velocities, and discharge, upon which a perfectly satisfactory professional opinion could be based. There was however considerable information among the older residents of the locality bearing more or less directly upon the question at issue, and there was one important item of precise knowledge which gave the extreme height of the flood of 1865 at two points near the highway on the north bank. One of those marks was made by Mr. Parshall after the water had reached its highest stand, upon the brick wall of the house in which he lived. The mark, with the date of the flood, is perfectly distinct at the present time, and the wall itself is stained by the turbid water as high as submerged, showing a sharp and well defined flood line. Distinct flood marks are seen at other points on Mr. Parshall's premises to the identity of which with the flood of 1865 he bears testimony.

At a distance of 2400 feet up stream from Parshall's the flood line of 1865 was marked by Mark Baldwin, on a large tree standing on the roadside near his house. The mark was made with an axe and is quite plain at the present time, and as Mr. Baldwin, who is now living, asserts its genuineness, there seems to be no occasion for doubt on this point. A straight line drawn through this tree at right angles to the direction of the stream will intersect the Lackawanna line near the western abutment of the Erie bridge.

The difference of level between the flood marks at Parshall's and Baldwin's is 1.73 feet which determines the surface slope of that flood to have been 3.806 feet per mile, or .0007208. This was on the flat where the water was shallow. The slope could not have been less than this in the main stream. On the contrary it would naturally be expected that the flood slope would be at least as steep as that of low river which is four feet to the mile, and certainly as steep as that of the overflowed bottom lands of the Valley, on the line of its general axis, which is known to be more than four feet to the mile.

Discharge.—The calculations for discharge will be applied to the profile at Parshall's house as determined by the flood of 1865, the width of water way being 3200 feet and its area 24,235 square feet, and the ascertained slope of 3.806 feet per mile will be regarded as existing at this profile, although strictly speaking, it belongs to the length of 2400 feet between Parshall's and Baldwin's.

This profile may properly be divided into two compartments, viz; a deep compartment embracing the bed of the Chemung River, and a shallow compartment covering the flats inundated during that flood.

The following quantities are known.

	Width.	Area	Hydra'lic Radius.
Deep Compartment.	500 ft.	10,752 sq. ft.	21.504 ft.
Shallow Compartment. ...	2700 "	13,483 " "	4.9937 "
Total....	3200 "	24,235 " "	

Surface Slope=.0007208

The hydraulic radius should be obtained by dividing the area of the profile by the length of wetted perimeter. In the present case that perimeter is so nearly identical in length with the width, that the latter is used to determine the value of the radius.

To find the volume of discharge, the

mean velocity with which the water flows through a given area of cross section must be known. Various formulas for this purpose have been constructed by scientists, some of them being simple in form, while others are more or less complicated. All of them so far as known require the slope of the water surface to be given, and also, directly or indirectly, the hydraulic radius.

One of the most simple of these formulas is that of D'Aubuisson, rendered into English measure by Downing. If

v = mean velocity in feet per second
R = hydraulic radius in feet, and
S = slope of water surface, then

$$v = C \times \sqrt{R \times S}$$

Using the values given above for the two compartments, the following results are obtained by means of the Downing formula, assuming the co-efficient C equal to 100 in the bed of the stream, and 84.3 according to Young, in the shallow compartment on the flats.

	Mean Velocity	Volume of Discharge
In the Deep Compartment	12.4500 ft.	133,865 cub. ft.
In the Shallow Compartment	5.0577 "	68,194 " "
Total Discharge..		202,059 " "

$$\text{Mean Velocity} = \frac{202{,}059}{24{,}235} = 8.3 \text{ feet per second.}$$

Among the foregoing deductions both the mean velocity in the deep compartment, and the aggregate volume of discharge attract attention by their magnitude. It will naturally be asked will the Chemung River, which is an insignificant stream in times of ordinary dry weather, flow with a mean velocity of 5.7 miles per hour, and discharge two hundred thousand cubic feet of water per second during a twenty-foot rise, equal to one-fifth the flood discharge of the Mississippi at New Orleans?

With regard to the volume of discharge it may be said that at the time of the rise referred to, the ground was frozen, and was covered with a heavy layer of snow. The weather suddenly became quite mild, and the snow began to melt rapidly, and this melting was accelerated and the flood was increased by a heavy fall of rain. For a few hours the volume of water carried into the stream was comparatively very large. Indeed if the volume which reached the stream during each of the four hours while the maximum height was maintained, amounted to one-tenth of an inch, or to be precise, to one hundred and four thousandths ($\frac{104}{1000}$) of an inch in depth over the whole area of water shed, it would account for the entire calculated discharge of 202,059 cubic feet per second. There would therefore seem to be no difficulty in regarding this as the actual discharge while that flood was at its maximum height, and as likely to occur at any time under a similar combination of circumstances.

In comparing the Chemung River with the Mississippi it should be remembered that although the flood section of the former is only about one-eighth that of the latter, its surface slope, on which the velocity and therefore the volume of discharge directly and largely depend, is about thirty-five times as great.

With regard to the computed mean velocity of 12.45 feet per second, or nearly 8.5 miles per hour in the bed of the stream, corresponding to a bottom velocity of 7.7 to eight feet per second, it may safely be said, from an examination of the tailings of bars and shoals, and from the statements of men who have resided on the banks of the Chemung River for many years, that large rounded stones of various shapes and sizes, very many of them weighing as much as thirty to fifty pounds, and measuring from eight and a half to ten inches in diameter, are carried down stream by floods of less magnitude than that of 1865. It is well known that they are moved more or less whenever the stream runs bank-full or nearly so.

This point although not discussed at any length in the testimony before the Commissioners, seems to be well established. These stones are not merely left in their places by the washing out and removal of the finer materials, they are positively moved more or less by every high freshet, as shown by the circumstance that the tailings are lengthened by extension down stream from year to year, the comparisons being made at the same stages of low river in successive seasons. On occasions of high freshet the stream becomes a not very mild type of mountain torrent.

The question in this connection is, What bottom velocity is necessary to transport rounded stones of from thirty to fifty pounds weight ranging from eight to ten inches in diameter?

It is believed that the following observations, even if they fail to convey any new information or develop any new facts, will at least show that our present knowledge on this subject is not only meager in amount but unsatisfactory and inconclusive in character.

MOVEMENT OF STONES IN RIVER BEDS.

The various rules or formulas referring to the motion and effects of flowing water, in river beds, are merely approximations in the matter of general application. Some of the more modern formulas seem to give results that agree more closely with observed facts than the older ones, so far as the mean velocity is concerned, but the determination of bottom velocities and of the weight and size of gravel, shingle and stones moved along the river bed by the bottom currents, are still matters of much doubt. In rivers of considerable depth it is generally impracticable to get stationary current meters sufficiently near to the bottom to give the real bottom velocities, and river reaches of even a few hundred feet in length will rarely be found of such uniform longitudinal section, that submerged floats will throughout the whole length pass close to the bed of the stream.

It is probable also that the bottom velocity varies greatly with the depth, although the mean velocity may remain nearly the same. The larger volume of water passing through a deep profile may flow with a more uniform motion, or with less marked differences between maximum and minimum velocities, than through a shallow profile. The current observations on the Mississippi River, have, in a number of cases, even shown the greatest velocity to be at, or near the bottom.

Generally speaking, engineers assume that the bottom velocity is less than the mean. A few rules have been applied to determine approximately the bottom velocities that may have existed in the Chemung River during the high flood of 1865.

1. Prony gives a formula which rendered into English terms, reads

$$\frac{m}{v}=\frac{v+7.71}{v+10.28}$$

in which m=mean velocity in feet per second.

and v=surface velocity in feet per second.

We deduce from this the surface velocity to be

$$v=\frac{m-7.71}{2}+\sqrt{\left(\frac{m^2}{4}+6.425\times m+14.86\right)}.$$

According to Rankine the bottom velocity can be assumed to be as much less than the mean velocity, as the maximum or surface velocity is greater than the mean. Therefore, if b represents the bottom velocity, we have $b=2m-v$.

According to this rule, the maximum velocity in the deep compartment in which the mean velocity was computed to be 12.45 feet per second, would be 13.9 feet, and the bottom velocity about eleven feet per second, or 7.5 statute miles per hour.

In the deepest portion, where there is nearly or quite 24 feet depth of water, the mean velocity is computed at about 13 feet, corresponding, by the rule, to a bottom velocity of 11.5 feet, or 7.84 miles per hour.

2. Du Buat's formulas, are as follows: Let

a=the observed surface velocity,

b=the bottom velocity,

c=the mean velocity; all in inches per second,

then

$$a=\left(\frac{1}{2}+\sqrt{C-\frac{1}{4}}\right)^2$$

$$b=(\sqrt{a}-1)^2$$

$$c=\frac{a+b}{2}$$

With an average velocity of 12.45 feet per second in the deep compartment, the above rule gives a bottom velocity of 11.4 feet or 7.77 miles per hour; or nearly 12 feet per second, equal to 8.15 miles per hour in the deepest portions.

3. In Trautwine's Civil Engineers' Pocket Book 1872, pp. 562 and 563, the following rule is given to find approximately the velocity at the bottom of a stream:

Rule: Multiply surface velocity by a certain coefficient given in table 15 to find the mean velocity. Multiply this mean

velocity by 2. From the product subtract the surface velocity; the remainder will be approximately the bottom velocity.

If we express the rule algebraically, let

V_s = surface velocity,
V_m = mean "
V_b = bottom "
x = variable coefficient; then

$$v_s = \frac{v_m}{x}; \text{ and}$$

$$v_b = 2 \times v_m - \frac{v_m}{x} = v_m \times \left(\frac{2x-1}{x}\right)$$

Applying the above rule, we find for the deep compartment a surface velocity of 14 feet, and a bottom velocity equal to 9.09 feet per second, or 6.2 miles per hour. In the deepest part of the section there should be found a bottom velocity of 11.55 feet per second, or 7.87 miles per hour.

4. G. Hagen, from his investigations, thinks that the mean velocity may generally be found actually to exist at about $\frac{4}{5}$ of the whole depth above the bottom of a river. He develops a formula for the mean velocity which is materially different from others, inasmuch as he introduces the 6th root of the slope.

If V=mean velocity in feet per second,
R=hydraulic radius (or mean depth approximately),
S=slope;

his rule, in English terms, is

$$V = 4.39 \times \sqrt{R} \times \sqrt[6]{S}$$

His velocity curve, in a perpendicular, is a parabola, with the vertex at the bottom of the river.

If C=bottom velocity, and
p=parameter of the parabola,

then he finds $\sqrt{p} = 0.225 \times C$;

$$V = C + \tfrac{2}{3}\sqrt{p} \times \sqrt{R};$$

$$\text{and } C = \frac{v}{1 + 0.15 \times \sqrt{R}}.$$

In applying the last rule to the case of the Chemung river, less values are obtained than by the previous methods. For the deep compartment, with a mean velocity of 12.45 feet per second, the bottom velocity is computed at 7.37 feet per second, or 5 miles per hour, and in the deepest portion a bottom velocity of 7.5 feet per second, or 5.11 miles per hour.

5. It is thought that the preceding rules, with the exception of that of Hagen, give excessive values for bottom velocities. If they had existed during such a flood as that of the Chemung river in 1865, they would have been more destructive than they were. Probably, nothing but a rock bottom could have resisted such currents.

The numerous and carefully-conducted current observations made in the Connecticut river by Gen. T. G. Ellis, under direction of Gen. G. K. Warren, U. S. Engineers, described in detail in the annual reports of the Chief of Engineers, U.S.A., for the years 1875 and 1878, show as an average result of all meter and float observations, for various velocities, a proportion of mean to bottom velocity of 193 to 100. Exactly the same result was obtained by current observations made about ten years ago on the river Elbe, in Bohemia, a brief record of which is given in the June number of VAN NOSTRAND'S ECLECTIC MAGAZINE for 1877.

As an average of velocities higher than 1.86 feet per second, the experiments with submerged floats on the Connecticut river showed the bottom velocity to be about 62 per centum of the mean.

Adopting the latter ratio, and it is found that the average bottom velocity in the deep compartment of the Chemung river would have been 7.72 feet per second, or 5.26 miles per hour, and in the deepest portion 8.06 feet per second, or 5.5 miles per hour.

Theoretically, the force of the impact of currents upon loose material obstructing the flow of water is proportional to the square of the velocity of these currents. The resistance of stones of the same kind, of approximately regular shape, spherical or cubes, is in proportion to their weights in water, or as the cubes of their diameter or sides.

If V, v are the velocities of currents, and and W, w the submerged weights of stones, the resistance of which will just be overcome by those currents, then

$$V^2 : v^2 = W : w.$$

If these stones are of similar and somewhat regular form their weights will be as the cubes of similar dimensions D, d;

i.e., of their diameters when round, and of their sides when cubes, therefore

$$V^2 : v^2 = D^3 : d^3.$$

The size and weight of a stone of certain form being known, that will just be moved from its place by currents of known velocity, the above rules would furnish the means of computing the size and weight of a stone of the same specific gravity and form, that would be moved by a different velocity. The data, however, on which to base calculations of that kind are exceedingly restricted in number, and are in some degree contradictory.

Almost every work on Hydraulics refers to Du Buat, who made a number of experiments on a small scale, from which it is believed to be unsafe to make deductions concerning phenomena developed by the currents of a large stream, upon a bed of material varying greatly in size, form and specific gravity. The following abbreviated table, taken from Rankine's Manual of Civil Engineering, shows the greatest velocities of currents close to the bottom, consistent with the stability of various material (according to Du Buat):

1. Gravel, 1 inch in diameter..... .....2.25 feet per second.
2. Pebbles, 1¼ inch in diameter.....3.33 feet per second.
3. Heavy Shingle.....4.00 feet per second.
4. Soft Rock, Brick and Earthenware.....4.50 feet per second.
5. Rock, various kinds..6.00 feet and upwards

The greatest bottom velocity in the Chemung river (12 feet per second) was computed according to Du Buat's formula (No. 2). A round flint pebble of 1½ inch diameter and 2.6 specific gravity, weighs in water about $\frac{1}{10}$ pound. According to the rule currents flowing with a velocity of 12 feet per second, or 8.15 miles per hour would scarcely be able to set in motion a rounded stone a little over 2½ inches diameter, weighing 1⅓ lbs. in water and 2½ pounds in air. It is well known, however, that currents of less strength do move far heavier stones; and it is evident that Du Buat's table is of little use as a basis for calculations involving velocities greater than a few feet per second.

Beardmore gives the following table:

Velocity of 6 inches per second does not disturb clay, with sand and stone;
Velocity of 8 inches per second moves coarse sand;
Velocity of 1 foot per second moves fine gravel, size of peas;
Velocity of 2 feet per second moves 1-inch rounded pebbles;
Velocity of 3 feet per second moves angular stones about 1¾ inch diameter.

Assuming in the last-named case, the stone, moved by a velocity of 3 feet per second, to be of approximately cubical shape, its weight would be about ½ pound in air and $\frac{3}{10}$ pound in water. By the theoretical rule cited above, a maximum bottom velocity of 12 feet per second would only be able to set in motion a cubical stone measuring 4.4 inches on each side, weighing a little over 8 pounds in air and about 5 pounds in water. This result is considered to be still far below what would be the actual fact with such an excessive velocity, as long as the stones are practically isolated from each other, so as to derive no support and increased resistance from being together in large masses and in close juxtaposition.

More recent observations of the power of currents to move stones are briefly recorded in the report of Engineer-in-Chief Jaquet on the improvement of the River Rhone from Lyons to the sea, dated July 1, 1878. He states that Chief Engineer Sainjon found that in the River Loire gravel and stone were moved as follows (here expressed in English terms of weights and measures):

Velocities in feet per second, 1.64. 3.28, 4.92, 6.56.
Diameters of stones in inches, .40 1.60, 3.90, 6.70

This little table substantially agrees with the formula expressing resistances of surfaces and bodies under water, originally, it is thought, given by D'Aubuisson, to wit:

$$R = C \times \frac{v^2}{2g} \times A \times w, \text{ in which}$$

R = resistance of the body to motion,
v = velocity in feet per second,
g = force of gravity at the place,
A = area of cross section of the body in square feet,
w = weight of a cubic foot in water,
C = coefficient, supposed to be constant for the same form of solid, but varying for different forms.

The resistance therefore varies as the square of the velocity; and with equal velocities, as the area of cross section.

Instead of $\frac{w}{2g}$ we use the co-efficient, $971\left(=\frac{62.5}{64.36}\right)$; therefore, $R=.971\times C\times a\times v^2$.

For a square plate, C is said to be equal to 1.43; for a cube, 1.21. To determine its value for a sphere (rounded stone) we assume that Sainjon's statement is correct: i.e., that a stone of 6.7 inches diameter is moved by a velocity of 6.56 feet per second.

On this basis we have C=.8896.

It has been already suggested that the bottom velocities found by the older formulas are too large. It is deemed to be much more within the limits of probability to adopt the results based on the experience lately had on the Connecticut River.

We find then that with a bottom velocity of 7.72 feet per second, stones of $9\frac{1}{4}$ inches diameter, and weighing thirty-nine lbs., may be moved; and with a maximum velocity of 8.06 feet in the most rapid places, the moving stones may weigh as much as 51lbs.

The preceding calculation of sizes and weight of stones, liable to be moved by the bottom currents during high floods, are based upon the velocities found by the D'Aubuisson-Downing rule. To compare results, velocities found by a few other formulas will now be used for computation.

By Darcy-Bazin's formula, the average velocity in the deep compartment was determined to be 12.35 feet per second, 62 per cent. of which, or 7.66 feet, would be the average bottom velocity. In the deepest places, with 24 feet of water, the mean velocity in the vertical plane would be 13.15 feet, with a bottom velocity of 8.15 feet. (For a depth of 24 feet, Darcy-Bazin's co-efficient for $\sqrt{R\times S}$ is a trifle over 100.

These bottom velocities would probably move stones measuring $9\frac{1}{8}$ to $10\frac{1}{4}$ inches diameter, weighing from 37 to 53 lbs. respectively.

By the Mississippi formula of Humphreys and Abbot, the average velocity in the deep compartment of the river was found to be 7.733 feet, supposed to correspond to a bottom velocity of 4.79 feet, which would be able to move stones of $3\frac{9}{16}$ inches diameter, weighing 1.37 pound. The velocity in the deepest section, at the bottom, would be 5.09 feet, capable of moving round stones of 4 inches diameter, and $3\frac{1}{4}$ lbs. weight.

According to the formula of Gauquillet and Kutter, the general average velocity in the deep compartment of the Chemung, would be 10.0122 feet per second, with a probable bottom velocity of 6.2 feet. In the deepest part, the mean velocity is computed to be 12.2 feet, and 7.56 feet at the bottom. These bottom velocities may move stones of from 6 to $8\frac{7}{8}$ inches diameter, weighing from $10\frac{1}{2}$ to 34 pounds.

From the foregoing, it appears that the movement of rounded stones in the bed of the Chemung River, measuring from $8\frac{1}{2}$ to 10 inches in diameter, and weighing from 30 to 50 lbs. is entirely consistent with the calculated discharge of 133.865 cubic feet per second, in the deep compartment of the stream, and the bottom velocities corresponding thereto.

It has been observed upon the Mississippi and Missouri Rivers that at the moment when the rising water overflows the banks and spreads over the bottom lands there is a reduction of velocity in the bed of the stream. A deposition of silt, and a building up of the bottom is the natural result of such action. The water from the bottoms flows into the stream at sundry places, bringing large quantities of silt with it. The same phenomena was probably developed in the Chemung River, the bottom being covered, for a time, with a layer of comparatively small gravel and pebbles, which were removed when the water subsided to the height of the banks. If so, the larger stones would be protected by a covering of smaller materials during the period of bank overflow, moving only while the water was within the banks, both on the rising and the falling stage.

INCREASED HEAD OR BANKING UP OF THE WATER.

To make an approximate computation of the head, probably produced by confining the water-way to the openings afforded by the two proposed Lackawanna bridges, it is assumed that these struc-

tures are located in an average profile of 3700 feet width, and 31,538 square feet area of cross section. In other words, for purposes of computation the Lackawanna Railroad is assumed to cross on a line of embankment and bridges located at right angles to the axis of the Valley, with a clear opening of 756 feet.

The average velocity of the water flowing through this average or imaginary profile during the highest flood, would be

$$v=6.4 \text{ feet per second, } \left(\frac{202{,}059}{31{,}538}\right)$$

The average hydraulic radius of this profile would be 8.5238 feet $\left(\text{or } \frac{31{,}538}{3700}\right)$

As to the reduced area of water way, an inspection of the flood profiles of the openings afforded by the two bridges has given the following approximate data:

	Effective width.	Area.	Hydra'lic Radius.
	feet.	sq. ft.	feet.
Chemung bridge.	576	9,868	17.132
Cove bridge.....	180	3,200	17.770

The difference between the two radii is so small that a common radius or mean depth for both bridge openings may be assumed without material error. The radius or mean depth at both bridges will therefore be taken at 17.2857 feet, $\left(\frac{9868+3.200}{576+180}\right)$

Works on hydraulics give different formulas for determining the amount of head produced in front of bridges and similar obstructions. Eytelwein gives the following:

$$y=\frac{v^2}{m^2}\times\left(\frac{W^2\times h^2}{w^2(h+y)^2}-1\right):$$

in which, v=mean velocity in original profile, in feet per second.

y=Increased head or banking up, in feet;

W=original width of profile, in feet;

w=reduced width of profile, in feet;

h=original mean depth of profile, in feet;

$\frac{1}{m^2}$=coefficient for contraction.

It is stated that where the contraction is produced by bridge piers, experience has shown that $\frac{1}{m^2}$ is about equal to 0.017, and this value will be used, although in the case before us the construction is the result of a combination of piers and solid embankments.

To apply the formula to the present case, the term h, in the denominator must be changed to h_1, representing the greater mean depth existing under the bridges, these having been located over the low river water ways, and where, therefore, by far the greatest depths are found at the flood stages.

The modified formula reads then:

$$y=\frac{v^2}{m^2}\times\left(\frac{W^2\times h^2}{w^2\times(h_1+y)^2}-1\right)$$

v=6.4065 feet,
W=3700 feet,
w=756 feet,
h=8.5238 feet,
h_1=17.2857 feet,
$\frac{1}{m^2}$=0.017 feet.

We find y=2.4272 feet . . . . (1).

How far up the stream the banking up of water caused by the bridges may extend, cannot be determined with accuracy, not only because the configuration of the flood-bed is imperfectly known, but also because the whole question has not yet been satisfactorily settled in theory. For common river courses of nearly uniform cross-section, D'Aubuisson says that the rise of water above the original slope-line will be sensibly felt to a distance up stream expressed by the equation:

$$D=1.9\times\frac{y}{s}$$

in which D=distance in feet,
y=head at obstruction in feet,
s=original slope.

If we assume that the slope above the profile is the same as between Baldwin's and Parshall's, then the distance will be 6398 feet or 1.21 miles.

Another solution of the problem of the amount of head formed at bridges is given in A. Debauves' *Manuel de l'In-*

genieur des ponts et Chaussies, 10*d Fascicule, ponts en Maconnerie.*

The formula is as follows:

$$y=\frac{Q^2}{2g}\times\left(\frac{1}{m^2\times w^2\times h^2}-\frac{1}{W^2\times(h+y)^2}\right)$$

In this formula, m is a co-efficient for contraction which has the following values, according to Gauthey:

$m=0.95$ when the piers present an acute angle to the current;
$m=0.90$ when the piers are rounded off to a demi-circle;
$m=0.85$ when the piers have square heads;
$m=0.70$ when the pier arches are small and more or less submerged.

The other terms represent the following values:

Q=volume of discharge, in cubic feet, per second.
$2g$=64.36 or double the acceleration of falling bodies at the end of 1st second.
W=original width of profile, in feet;
w=reduced width of profile, in feet;
h=mean depth of original profile, in feet;
y=increased head or rise in feet.

To apply this formula to our case, h in the first member or fraction must be changed as before to h_1, representing the greater depth under the two bridges. We then have

$$y=\frac{Q^2}{2g}\times\left(\frac{1}{m^2\times w^2\times h_1^2}-\frac{I}{W^2\times(h+y)^2}\right)$$

Q=202,059 cubic feet,
W=3700 feet,
w=756 feet,
h=8.5238 feet,
h_1=17.2857 feet,
m=0.9.

We get y=4.3168 feet=increased head at bridges . . . . (2).

$$D=11{,}378\text{ feet}\left(=\frac{1.9\times4.3168}{.0007208}\right)$$

=2.16 miles, the distance above the obstruction at which the increased rise will cease to be felt.

ADDITIONAL CALCULATIONS.

In the foregoing calculations, Downing's formula for determining the mean velocity was used to obtain the discharge. For comparison, three other formulas will now be tried, namely that of Bazin, then the Humphreys-Abbot or Mississippi formula, and finally that of Gauquillet and Kutter.

BAZIN'S FORMULA FOR VELOCITIES.

For river beds in their natural condition, the value v (velocity in feet per second) expressed in English terms of measure, is as follows:

$$v=\frac{\sqrt{R\times S}}{v\left\{.00008534\times\left(1+\frac{4.1013}{R}\right)\right\}}$$

R=hydraulic radius in feet,
S=slope.

Applying this formula to the profile at Parshall's, we have

In the deep compartment v=12.3503 ft.; volume . . . . =132,790 cub. ft.
In the shallow compartment v=4.8122 ft.; volume . . . . = 64,883 "

Total volume of discharge per second=197,673 "

With a volume of 197,673 cubic feet, we obtain:

3. *Increasec rise in feet*, by Eytelwein's rule, y=2.3474 feet.

Extension of rise, up stream, D=6.187 feet (=1.17 mile.)

4. *Increased rise in feet*, by Debauve's rule, y=4.1113 feet.

Extension of rise, up stream, D=10,837 feet (=2.05 mile.)

Humphreys and Abbot's formula, in its most simple form (see page 29, notes on practical gauging of rivers No. 2 printed papers, by Gen. H. L. Abbot, April 13, 1868,) gives the following equation for velocity v.

$$v=\left\{\sqrt{m+\left(\frac{225\times a\times\sqrt{S}}{p+W}\right)^{\frac{1}{2}}}-\sqrt{m}\right\}^2$$

m=co-efficient varying with the hydraulic radius;
a=area of cross section, in square feet;
S=slope;
W=width at water surface;
p=wetted perimenter (approximately equal to $1.015\times W$,)

For the profile at Parshall's we get:

	Radius.	m.	$\sqrt{m}$.	Area.	W.	p.	Velocity.	Volume.
	feet.			sq. ft.	ft.	feet.	feet.	Cub. feet
Deep compartment	21.5040	.002825	.0531	10,752	500	507.5	7.7330	83,150
Shallow compartment	4.9937	.005400	.0733	13,483	2700	2740.5	3.5914	48,423
Total								131,573

With this aggregate volume of 131,573 cubic feet we obtain

5. *Increased rise in feet*, by rule in Eytelwein's work, $y=1.2092$ feet.

Extension of rise, up stream, D= 3,187 feet ($\frac{6}{10}$ mile.)

6. *Increased rise in feet*, by rule in Debauve's work, $y=1.7587$ feet.

Extension of increased rise up stream, D=4,636 feet (or $\frac{3}{5}$-mile.)

The rule for determining the mean velocity of water in rivers, given by *Gauquillet and Kutter* for Austrian measures, is as follows:

$$v=\left\{\frac{41+\frac{1.779}{n}+\frac{.00276}{s}}{1+\left(41+\frac{.00276}{s}\right)\times\frac{n}{\sqrt{R}}}\right\}\times\sqrt{R\times S}$$

S=slope of water surface;
R=hydraulic radius;
n=.026 for large rivers.

In using this formula it must be remembered that 1 ft. Austrian=1.0371 ft. English.

For the profile at Parshall's, we obtain:

Deep compartment, velocity=10.0122 feet;
volume . . . . . =107,650 cub ft.
Shallow compartment,
velocity=3.9576 feet;
volume . . . . . = 53,360 "
Total volume . . 161,010 "

7. *Increased rise in feet*, by rule in Eytelwein's work, $y=1$ 6968 feet.

Extension of increased rise up stream, D=4,472 feet, =.847 mile.

8. *Increased rise in feet*, by rule in Debauve's work, $y=2.6775$ feet.

Extension of increased rise up stream, D=7,057 feet=1.337 mile.

RECAPITULATION.

Formulæ for velocity.	Volume of discharge.	Formulæ for rise.	
		Eytelwein.	Debauve.
		feet.	feet.
Downing	202,059	2.4272	4.3168
Bazin	197,673	2.3474	4.1113
Humphreys-Abbot	131,573	1.2092	1.7587
Gauquillet and Kutter.	161,010	1.6968	2.6775

The widely varying results obtained by using different formulas for discharge and for banking up, as shown in the foregoing discussion, afford a striking proof of the imperfection of our knowledge on this subject, and the danger of placing too much reliance upon conclusions drawn from the application of fixed rules to the phenomena of river flow.

The inherent value of an empirical rule depends not only upon the degree of care with which the fundamental observations are made, but upon the intelligence with which those observations are interpreted.

Hydraulic formulas probably have a somewhat restricted range, and the local conditions under which they are applied in practice should differ as little as possible from those upon which they were originally constructed.

The depth of a stream, the longitudinal and transverse profiles, the surface slope, the tortuosity, the ratio of friction to volume of discharge and the character of the bed and banks, are all widely varying functions in different parts of the same stream, even while the water remains at a fixed stage. And it will perhaps be conceded, that a formula which shall satisfactorily express these complex conditions—supposing such a formula possible—may become inapplicable to a greater or less degree, when we pass to other streams, whether larger or

smaller, or to higher and lower stages of the same stream.

The results of the trial, as recommended by the Commissioners and adopted by the Justice of the Supreme Court of the State in rendering his decision, are briefly as follows:

1st The Lackawanna track shall cross the Erie track at a clear height of 20 feet above the latter, on a through bridge of 55 feet clear span, measured at right angles to the tangent to the Erie curve at the point of crossing.

2d. The bridge over Parshall's Cove, the solid embankment from said bridge to the south bank of the Chemung River, and the bridge over said river may be constructed as projected by the Lackawanna Company, while west of the western abutment of the Chemung bridge for a distance of 405 feet, bridging shall be constructed in spans of not less than 135 feet. The height of the bridging at the Cove, at the Chemung River and westerly therefrom shall be not less than 20 feet above the low water of the river.

3d. The material under all the bridge spans, except that over the Erie track, is to be excavated to the level of low water.

The court awarded the sum of fifteen hundred and one dollars to the Erie Railroad Company, to be paid by the Lackawanna Company, such being the amount of the damages to the first-named company as estimated by the Commissioners.

Of the above sum, one dollar was to be paid for the use of the Erie Company's right of way at the point of crossing; $500, as compensation to that Company for necessary expenses in taking certain precautions while the bridge spanning its tracks is in course of construction; and the sum of $1000 for expenses to be incurred by the Erie Company by being obliged to rip-rap the side slope of its road at and near the crossing, as the Commissioners judge that the construction of the works contemplated by the Lackawanna Company will cause an increase of the currents of the flood waters of the river in that locality, to such an extent as to render the said water slope of the Erie Road liable to erosion by the water.

According to the ruling of the court the area of cross section, available for the passage of the flood waters of the river, will be sensibly larger than it was assumed in the foregoing calculations

The river bridge will be actually extended by the bridge openings west of it, for a distance of 405 feet; and since the materials under the bridge spans are to be excavated to the level of low water, a flood area of several thousand square feet will be added to the estimated amount.

Under these circumstances the increased head of water that will be produced above the bridges will, of course, be less than that computed by either one of the formulas applied.

PRACTICAL REMARKS ON THE SEASONING OF WOOD.

From "The Builder."

THERE have been promulgated from time to time so many different theories concerning the best way of seasoning wood, that, whilst taking the subject once more in hand, we feel that we are not only approaching debatable ground, but that we are stepping on to that which has been already well-nigh exhausted by writers.

Let us commence this paper by remarking that wood is an elastic material; that it expands under the influence of moisture; and that it contracts again when by the process of drying, known as "seasoning," the moisture has been evaporated from it. A thin coating of paint or other material, when applied to seasoned wood, may be the means of keeping out moisture, and, of course, if it be kept out, the wood will not be in any way affected by it; but if the moisture be permitted to approach dried wood, the sponge-like character of the material will absorb it, and it will expand in proportion to the amount of moisture that has been absorbed.

We have remarked that a coating of paint or other similar material will, when

applied to wood, exclude moisture, and we may further remark that by the action of the evaporation of the natural juices of wood, it would appear that a sort of fine skin is given to the wood, which skin is sufficient, frequently of itself, to exclude moisture. We will furnish an example of this fact by detailing a case which came under our own observation. The flooring boards of an old floor in a dwelling house were taken up for the purpose of being replaned. They had been laid down considerably over twenty years. Before being taken up the joints were seen to be quite close fitting. After having been planed over the boards were relaid, and with the result that in about two months all the joints had considerably opened, and it was found to be necessary to again take up the boards and relay them, so as to secure close-fitting joints. It was the fact that by the removal of the old skin evaporation had recommenced, so the wood had undergone a second process of shrinking.

Wood requires time in which to season, very much in proportion to the density of its fiber. But this rule is not without an exception, for pitch pine, which is not at all a densely fibered wood, requires a long time in which to season, even when the process is conducted under favorable conditions.

This occurs probably in consequence of the resinous character of pitch pine, the resin clogging the pores of the wood, and thus stopping up the channels through which the moisture would otherwise exude. There are some woods.—and mahogany, ebony, and some other of the tropical woods are of the number—that even in their living state contain very little moisture.

Plants that are of slow growth contain less moisture when in a living state than do those whose growths are rapid. A mahogany tree requires 500 years in which to mature, and, as a consequence, its texture is exceedingly dense. Being dense in texture, it requires a long time to properly season, and during that lengthened period it shrinks very little. Mahogany should not be kept longer than necessary in the log, because insomuch as the outside portion of a log contains the greatest amount of moisture, and it being the exposed part, it will, as the wood dries, shrink more than the inner wood, and so, to allow for the outside shrinking, outside shakes will and must occur.

The same remark applies with equal force to all log timber, but we name the circumstance in connection with mahogany particularly for the reason that it is a general practice for some to keep their mahogany logs in an unsawn state, under the misapprehension that the logs do not deteriorate. When it is required to keep the logs in comparative bulk, it will be found to be a convenient method to have one cut put down the centre of them, which, as a rule, will be sufficient to obviate any tendency to outside shake that may arise in consequence of their shrinking on the outside. When cut mahogany boards should always be laid aside to season in the same order as they have left the saw. Strips of accurately-sawn wood should be placed at intervals of a not greater distance than 12 inches from each other, and we are inclined to advise that the strips of wood be placed even closer together than that.

It is advisable that some woods should be seasoned quickly, and others should dry slowly. Mahogany must be seasoned slowly. To season it thoroughly and well, periods should be allowed in something like conformity with the following table. Care should also be taken that the strips of wood which separate each board should have their positions changed at least once in each year, and the whole of the boards should have their sides reversed once in each year, so that the sides of the boards which were below should, by being reversed, be placed upwards, and *vice versa*. The remarks made concerning the seasoning of mahogany boards may be taken to apply also to wainscot oak boards.

TABLE APPORTIONING THE TIME TO BE ALLOWED FOR SEASONING MAHOGANY AND WAINSCOT OAK BOARDS UNDER FAVORABLE CONDITIONS:

Thickness of Boards.	Time to be allowed for seasoning in the open.
¾ inch	12 months.
1 "	24 "
1¼ "	30 "
1½ "	33 "
2 "	36 "
3 "	46 "
4 "	56 "

It is, perhaps, superfluous to add that some kind of improvised roof must be provided under which to shelter the seasoning board from the damaging effects of the sun, the rain, and dirt. It is the practice with some to preserve the ends of mahogany boards, when they are undergoing the process of seasoning, by nailing a strip of wood on the end of each board. Of this practice we do not altogether approve, for the reason that it retards, to some extent, evaporation. A roof which sufficiently overhangs the ends of the boards is a complete preventive, and is more advisable.

Newly-felled pine timber contains about 39 per cent. of moisture. The specific gravity of pine is (to water= 1,000) 0.9121. It can be air-dried to 0.5502, and artificially dried to 0.4200. The specific gravity of fir is 0.8700. It can be air-dried to 0.4700, and artificially dried to 0.3800.

Insomuch as pine deals reach this country in a narow range of length, they can be placed in a position for drying which they would not well occupy if the range of lengths was longer. It is the practice with many joiners to "perch" their pine boards to season; but we are opposed to the practice of placing the boards in any position where they become dirty, and impregnated with smoke, dirt, and grit. We hold that the object to be attained in the seasoning of wood is to slowly evaporate the moisture which is in the wood, without permitting the wood to lose its sawn shape, and at the same time to keep it perfectly clean.

It is here worth while penning a few notes relative to the importance attached to keeping wood clean, because the matter is one which is, now-a-days, receiving considerable attention. It is only of late that steamships have been largely employed to bring over foreign sawn timber. Shortly after the introduction of steamships into the wood-carrying trade, timber importers had the advantages which these vessels afford over sailing ships made clearly manifest to them, by the preference which consumers of wood showed for the excellent condition in which the wood brought over in steamships reached them. The quick passages made by steamers afforded no time for any kind of fermentation of the sap to ensue, and there being no sea-washed deck-cargo to spoil the general appearance of a parcel, the preference for clean-looking stock began to direct the attention of consumers to the advantage of keeping seasoned wood also clean. There is no greater mistake made than to suppose, as some do, that the bulk of wood in a timber-merchant's yard necessarily improves with time. Timber importers learn to their cost that, instead of improving, it deteriorates very quickly indeed; and one of the questions which many timber importers have set themselves to consider is, whether it is not advisable for them to incur the cost of erecting large sheds under which they can store their better quality deals, and thus keep them clean.

Pine boards should be seasoned on very much the same plan as that which we have recommended for the seasoning of mahogany boards. We suggest only this alteration, that they should be placed where the wind can reach them, for they should be dried quickly. The reason for this is that pine is a wood which is prone to rapid decay, and therefore the quicker it can be placed in an absolutely dry position—such for instance, as being made up into internal joinery work—the better.

Redwood is a difficult timber to season properly. The lack of uniformity in the lengths of redwood deals renders the adoption of the same plan as that which we have suggested for the seasoning of pine boards inconvenient, because insomuch as the long boards would hang over the others the leverage would be certain to cause the boards to bend. Perhaps the best way is to place them on their edges in a frame although in this situation they would be likely to twist somewhat. The general practice is to "perch" them, but if they be at all "shelly," and "shelliness" is a fault usually attached to the finer kinds of redwood—such as Archangel and Onega redwood, the rain is apt to penetrate into the "shelly" parts of the boards, and should frost succeed the rain it splits and rives open the boards terribly. It would thus appear to be imperative that redwood should be seasoned under cover. Wind is a useful agent to assist in the drying of redwood.

Baltic whitewood, of all classes,

seasons quickly, and as the wood rapidly deteriorates under the action of the rain, the sun, and frost, the quicker it is dried the better.

Whitewood cannot be satisfactorily dried by artificial means, for it twists under the action of heat. Whitewood hardens very much by being exposed to the weather. The reason of this is that from its sponge-like texture the pores of the wood rapidly close when evaporation ensues.

We can recognize no advantage in incurring the expense of seasoning bearing woods. Of course, if the wood is positively wet, it must be dried of the superfluous moisture, but that may be considered to be sufficient. Flooring boards are best seasoned by being "perched" in an elevated position, where the wind can freely circulate through them, and cause them to rapidly dry. The ends of the boards should be well lifted out of the damp earth. Perches of different heights should be erected, so that the long boards can be dried upon a high perch, and thus they are prevented from bending sufficiently as to cause them to dry in a twisted form. For the sake of preserving the face sides of the boards clean, it is a good plan to "perch" the boards in pairs by placing two boards with their face sides together. They will dry perfectly well when so placed, and no little advantage is secured by having the boards turn out clean on the face side, for in a number of cases it will be found unnecessary to re-plane them when laid. We have commented, in the commencing portion of this paper, upon the danger of flooring boards undergoing a second shrinking process on the removal of the dried faces of the boards.

It may here be remarked, and the comment is well worthy of the attention of architects, that the shrinking of flooring boards when laid is oftener to be accounted for by the fact, that after the floor has been laid the boards have been replaned up so as to give a clean new-looking surface, than for the reason that the boards have been insufficiently dried.

The practice of drying the boards with their face sides together secures, as we have remarked, clean faces, and thus avoids the necessity of having the boards replaned after they have been laid, and as a consequence reduces their liability to shrink.

We have indulged in repetition on this point for the purpose of giving emphasis to our views, regarding them as being of some importance.

The system of stacking flooring boards to dry in triangular fashion is much to be condemned for several sufficient reasons. The first is that, when so piled, boards will hold no inconsiderable quantity of snow or rain water.

The second is that they do not dry in those places where the boards must necessarily touch each other in the crossing.

The third reason is that when the boards are in long lengths they have a natural tendency to "swag" in their centers, and so to dry in a twisted or crooked form.

It is to be borne in mind that, although wood the grain of which is of a twisted character has a natural tendency to dry crooked, yet that it can, by being judiciously weighted, be kept straight.

This fact, however, suggests to us that even straight-grained wood will dry in a crooked form, if, when left to season, it be allowed to assume a bent form, and that this being so it is imperative that the piling of wood into position for seasoning should be directed by care and intelligence.

THE results of a third year's observation of spirit levers at Secheron, for elucidation of periodic movements of the ground, to which we have several times referred, are given by M. Plantamour in the December issue of *Archive des Sciences*, and Col. von Orff also communicates results obtained at the Observatory of Bogenhausen—three to four kilometers from Munich. M. Plantamour shows that the oscillations, both in the east-west and north-south directions, present anomalies, or differences from year to year, which cannot be explained by mere variations of the temperature of the air. The earth's surface he supposes to be in a state of constant gentle undulation, the direction and amplitude of which varies in each locality according to the nature of the ground and the forces in action; and the effect may strengthen or neutralize that of the air temperature on the ground, or even produce a movement in an opposite direction. Col. von Orff's observations, *Nature* says, afford ground for supposing that the spirit-level variations are, partly at least, caused by variations of heat in the formation on which the observatory rests.

MODERN ORDNANCE.

By COLONEL MAITLAND.

From the "Journal of the Society of Arts."

A GREAT change has lately been taking place throughout Europe in the matter of armaments. Artillery knowledge has been advancing "by leaps and bounds;" and all the chief nations are vying with each other in the perfection of their *materiel* of war. As a readiness to fight is the best insurance for peace, it behoves us to see from time to time how we stand, and the present moment is a peculiarly suitable one for taking stock of our powers and capabilities. I propose, therefore, to give you this evening, a brief sketch of the principles of manufacture of modern guns, at home and abroad, concluding with a few words on their employment and power.

The introduction of rifled cannon into practical use about 20 years ago, caused a complete revolution in the art of gun making. Cast iron and bronze were found no longer suitable for the purpose. Cast iron was too brittle to sustain the pressure of the powder gas, when its duration was increased by the use of elongated projectiles; while the softness of bronze was ill adapted to retain the nicety of form required by accurate rifling.

From among a cloud of proposals, experiments and inventions, two great systems at length disentangled themselves. They were the English construction of built-up wrought-iron coils, and the Prussian construction of solid steel castings.

Wrought iron, as you are all aware, is nearly pure iron, containing but a trace of carbon. Steel, as used for guns, contains from 0.3 to 0.5 per cent. of carbon; the larger the quantity of carbon, the harder the steel. Since the early days of which I am now speaking, great improvement has taken place in the qualities of both materials, but more especially in that of steel. Still the same general characteristics were to be noted, and it may be broadly stated, that England chose confessedly the weaker material, as being more under control, cheaper, and safer to entrust with the lives of men; while Prussia selected the stronger but less manageable substance, in the hope of improving its uniformity, and rendering it thoroughly trustworthy. The difference in strength, when both are sound, is great. Roughly, gun steel is about twice as strong as wrought iron.

I must now say a few words on the nature of the strains to which a piece of ordnance is subjected when fired. Gunpowder is commonly termed an explosive, but this hardly represents its qualities accurately. With a true explosive, such as gun cotton, nitro-glycerine and its compounds, detonation and conversion of the whole into gas is practically instantaneous, whatever the size of the mass; while, with gun powder, only the exterior of the grain or lump burns and gives off gas, so that the larger the grain the slower the combustion. The products consist of liquids and gases. The gas, when cooled down to ordinary temperature, occupies about 280 times the volume of the powder. At the moment of combustion, it is enormously expanded by heat, and its volume is probably somewhat about 6,000 times that of the powder. I have here a few specimens of the powders used for different sizes of guns, rising from the fine grain of the mountain gun to the large prisms and cylinders fired in our heavy ordnance. You will readily perceive that, with the fine grained powders, the rapid combustion turned the whole charge into gas before the projectile could move far away from its seat, setting up a high pressure which acted violently on both gun and shot, so that a short, sharp strain, approximating to a blow, had to be guarded against.

With the large slow bursting powders now used, long heavy shells move quietly off under the impulse of a gradual evolution of gas, the presence of which continues to increase till the projectile has moved a foot or more; then ensues a contest between the increasing volume of the gas, tending to raise the pressure, and the growing space behind the advancing shot, tending to relieve it. As artillery science progresses, so does the

duration of this contest extend farther along the bore of the gun towards the great desideratum—a low maximum pressure long sustained.

When quick burning powder was used for ordnance, the pressures were short and sharp; the metal in immediate proximity to the charge was called upon to undergo severe strains, which had scarcely time to reach the more distant portions of the gun at all; the exterior was not nearly so much strained as the interior. In order to obviate this defect, and to bring the exterior of the gun into play, the system of building up guns of successive tubes was introduced. These tubes were put one over the other in a state of tension produced by "shrinkage." This term is applied to the process of expanding a tube by the application of heat, and in that condition fitting it over a tube larger than the inner diameter of the outer tube when cold. When the outer tube cools it contracts on the inner tube and clutches it fast. The wrought-iron guns of England have all been put together in this manner.

Prussia at first relied on the superior strength of solid castings of steel to withstand the explosive strain, but at length found the necessity for reinforcing them with hoops of the same material, shrunk on the body of the piece.

The grand principle of shrinkage enables the gunmaker to bring into play the strength of the exterior of the gun, even with quick powders, and to a still greater extent as the duration of the strain increases with the progress of powder manufacture. Thus, taking our largest muzzle-loaders designed a few years ago, the thin steel lining tube, which forms an excellent surface, is compressed considerably by the wrought-iron breech coil holding it, which, in its turn, is compressed by the massive exterior coil. When the gun is fired, the strain is transmitted at once, or nearly at once, to the breech coil, and thence more slowly to the outer one. Now, as the duration of the pressure increases, owing to the use of larger charges of slower burning powder, it is evident that the more complete and effective will be the transmission of the strain to the exterior, and, consequently, the farther into the body of the gun, starting from the bore, and traveling outwards, does it become advantageous to employ the stronger material. Hence, in England, we had reason to congratulate ourselves on the certainty and cheapness of manufacture of wrought-iron coils, as long as moderate charges of comparatively quick burning powder were employed, and as long as adherence to a muzzle-loading system permitted the projectiles to move away at an early period of the combustion of the charge. Then, the pressures, though sharp, were of short duration, and were not thoroughly transmitted through the body of the gun, so that the solidity, mass, and compression of the surrounding coils proved usually sufficient to support the interior lining. Now that breech loading and slow powders have been introduced, these conditions have been changed. The strains, though less severe, and less tending to explosive rupture, last longer, and are more fully transmitted through the body of the gun. Sheer strength of material now tells more, and signs have not been wanting that coils of wrought iron afford insufficient support to the lining. It becomes, therefore, advantageous to thicken the inner tube, and to support it with a steel breech piece. Carrying this principle further, we shall be led to substitute the stronger for the weaker metal throughout the piece. This has been done by the Germans in the first instance, and recently by the French also. It is probable that we shall follow the same course. When I say "probable," I intentionally guard myself against uttering a prediction. It is never safe to prophesy, unless you know, as the American humorist puts it. And in this case we do not know, for a very dangerous rival, once defeated, but now full of renewed vigor, has entered the lists against forged steel as a material for ordnance. This rival's name is *wire*. Tempered steel wires can be made of extraordinary strength. A piece of round section, only $\frac{1}{12}$ of an inch in diameter, will just sustain a heavy man.

If, now, a steel tube, suitable for the lining of a gun, be prepared by having wire wound round it very tightly, layer over layer, it will be compressed as the winding proceeds, and the tension of the wire will act as shrinkage. You will readily understand that a gun can be thus formed, having enormous

strength to resist bursting. Unfortunately, the wires have no cohesion with one another, and the great difficulty with construction of this kind is to obtain what gun makers call end strength. It is of but little use, to make your walls strong enough, if the first round blows the breech out. In the early days of wire this was what happened, and Mr. Longridge, who invented the system, was compelled to abandon it.

Lately, methods have been devised in France, by M. Schultz; at Elswick, by Sir W. G. Armstrong & Co.; and at Woolwich by ourselves, for getting end strength with wire guns. They are all in the experimental stage; they may prove successful; but I prefer not to prophesy at present.

The diagrams on the wall show the general construction of the modern German, French and English heavy breech-loading guns. The Germans have a tube, a jacket, and hoops. The French, a thick tube or body and hoops. The English, a tube, a jacket, and an overcoat, as it may be called. In each system of construction, the whole of the wall of the gun comes into play to resist the tranverse bursting strain of the charge.

The longitudinal or end strength varies; thus, in the German guns, the tube and hoops do nothing—the jacket is considered sufficient. The French construction relies entirely on the thick body, while the English method aims at utilizing the whole section of the gun, both ways. Of course, if the others are strong enough, there is no particular advantage in this; and it is by no means improbable that eventually we shall find it cheaper, and equally good, to substitute hoops for the "overcoat."

I fear I have detained you a long time over construction, but it is both instructive and interesting to note that certain well-defined points of contact now exist between all the great systems. Thus, a surface of steel inside the bore is common to all, and the general use of steel is spreading fast. Shrinkage, again, is now everywhere employed, and such differences as still exist are matters rather of detail than of principle, as far as systems of construction are concerned.

We now come to a part of the question which has long been hotly debated in this country, and about which an immense quantity of matter had been both spoken and written on opposite sides—I mean muzzle loading and breech loading. The controversy has been a remarkable one, and, perhaps, the most remarkable part of it has been the circumstance that, while there is now little doubt that the advocates of breech loading were on the right side, their reasons were for the most part fallacious. Thus, they commonly stated that a gun loaded at the breech could be more rapidly fired than one loaded at the muzzle. Now, this was certainly not the case, at any rate with the comparatively short guns which were made on both systems a few year ago. The public were acquainted with breech-loaders only in the form of sporting guns and rifles, and argued from them. The muzzle-loading 38-ton guns were fired in a casemate at Shoeburyness repeatedly in less than twenty minutes for ten rounds, with careful aiming. No breech loader of corresponding size has, I think, ever beaten that rate. With field guns in the open, the No. 1 of the detachment can aim his muzzle loader while it is being loaded, while he must wait to do so till loading at the breech is completed. Again, it was freely stated that, with breech loaders greater protection was afforded to the gunners than with the muzzle loaders. This entirely depends on how the gums are mounted. If in siege works or *en barbette*, it is much easier to load a muzzle-loader under cover than a breech loader. But I need not traverse the old ground all over again. It is sufficient for me to say here, that the real cause which has rendered breech loading an absolute necessity is the improvement which has been made in the powder. You witnessed a few minutes ago the change which took place in the action of fired gunpowder when the grains were enlarged. You will readily understand that nearly the whole of a quick-burning charge was converted into gas before the shot had time to start; suppose for the moment that the combustion was really instantaneous. Then we have a bore, say 16 diameters long, with the cartridge occupying a length of, say, two diameters.

The pressure of the gas causes the shot to move. The greater the pressure, the greater the impulse given. As the shot advances, the pressure lessens; and

it lessens in proportion to the distance the shot proceeds. Thus, when the shot has proceeded a distance equal to the length of the cartridge, the space occupied by the gas is doubled, and its original pressure is halved. As the shot travels another cartridge length, the space occupied by the gas is trebled, and its pressure will be but one-third of the original amount. When the shot arrives at the muzzle—that is, at eight times the length of the cartridge from the breech—the pressure will be but one-ninth of that originally set up. Remember, this is on the supposition that the powder has been entirely converted into gas before the shot begins to move.

Now, suppose the powder to be of a slow-burning kind, and assume that only one-third of it has been converted into gas before the shot starts, then the remaining two-thirds will be giving off additional gas, as the shot travels through the bore. Instead, therefore, of the pressure falling rapidly, as the shot approaches the muzzle, the increasing quantity of gas tends to make up for the increasing space holding it.

You will at once perceive that the slower the combustion of the powder, the less difference there will be in the pressure exerted by the gas at the breech and at the muzzle, and the greater will be the advantage, in point of velocity, of lengthening the bore, and so keeping the shot under the influence of the pressure. Hence, all recent improvements, has tended towards larger charges of slower burning powder and increased length of bore. And it is evident that the longer the bore of the gun, the greater is the convenience of putting the charge in behind, instead of having to ram it home from the front. I may here remark, that the increased length of gun necessary to produce the best effect is causing even those who have possessed breech loaders for many years to re-arm just as completely as we are now beginning to do. All the old short breech-loading guns are becoming obsolete. Another great advantage of breech loading is the facility afforded for enlarging the powder chamber of the gun, so that a comparatively short, thick cartridge may be employed, without any definite restriction due to the size of the bore.

There is yet one more point in which breech loading has recently been found, in the Royal Gun Factory, to possess a great advantage over muzzle loading as regards ballistic effect. With a shot loaded from the front, it is clear that it must be smaller all over than the bore, or it would not pass down to its seat. A shot thrust in from behind, on the contrary, may be furnished with a band or sheath of comparatively soft metal larger than the bore; the gas then acting on the base of the projectile, forces the band through the grooves, sealing the escape, entering the projectile, and to a great extent, mitigating the erosion of surface. This is, of course, universally known. It is also pretty generally known among artillerists that the effect of the resistance offered by the band or sheathing on the powder, is to cause more complete combustion of the charge before the shot moves, and therefore to raise the velocity and the pressure. But, I believe, it escaped notice till observed in May, 1880, in the Royal Gun Factory, that this circumstance affords a most steady and convenient mode of regulating the consumption of the charge, so as to obtain the best results with the powder employed.

Supposing the projectile to start, as in a muzzle loader, without offering any resistance beyond that due to inertia, it is necessary to employ a powder which shall burn quickly enough to give off most of its gas before the shot has pro ceeded far down the bore; otherwise, the velocity at the muzzle will be low. To control this comparatively quick-burning powder, a large air space is given to the cartridge, which, therefore, is placed in a chamber considerably too big for it. Supposing, on the other hand, the projectile to be furnished with a stout band, giving a high resistance to initial motion, a much slower powder can be used, since the combustion proceeds as if in a closed vessel, until sufficient pressure is developed to overcome the resistance of the band. This enables us to put a larger quantity of slower burning powder into the chamber, and in fact to use, instead of a space filled with air, a space filled with powder giving off gas, which comes into play as the projectile travels down the bore. Thus, while not exceeding the intended pressure at the breech, the pressure towards the muzzle is kept up, and the velocity very materially increased.

Following this principle to this conclusion, it will be found that the perfect charge for a gun will be one which exactly fills the chamber, and which is composed of a powder rather too slow to give the pressure for which the gun is designed; supposing the shot to move off freely. The powder should be so much too slow as to require for its full development the holding power of a band which is just strong enough to give rotation to the shot.

Having settled that the gun of the future is to be a breech loader, we have next to consider what system of closing the breech is to be adopted.

The German guns are provided with a round-backed wedge, which is pushed in from the side of the breech, and forced firmly home by a screw provided with handles; the face of the wedge is fitted with an easily removable flat plate, which abuts against a Broadwell ring, let into a recess in the end of the bore. On firing, the gas presses the ring firmly against the flat plate, and renders escape impossible as long as the surfaces remain uninjured. When they become worn, the ring and plate can be exchanged in a few minutes. Mr. Vavasseur, of Southwark, constructs his guns on a very similar plan. In the French guns, and our modern ones, the bore is continued to the rear extremity of the piece, the breech end forming an intermittent screw, that is, a screw having the threads intermittently left and slotted away. The breech block has a similarly cut screw on it, so that when the slots in the block correspond with the untouched threads in the gun, the block can be pushed straight in, and the threads made to engage by part of a revolution. In the French Marine the escape of gas is stopped very much as in Krupp's system; a Broadwell ring is let into a recess in the end of the bore, and a plate on the face of the breech block abuts against it.

In the French land service the escape is sealed in quite a different manner. A stalk passes through the breech block, its foot being secured on the exterior. The stalk has a mushroom-shaped head projecting into the bore. Round the neck of the stalk, just under the mushroom, is a collar of asbestos, secured in a canvas cover; when the gun is fired, the gas presses the mushroom against the asbestos collar, and squeezes it against the walls of the bore, It is found that this cuts off all escape.

We are at present using the Elswick method, which consists of a flat-backed cup, abutting against the slightly rounded face of the breech plug. The lips of the cup rest against a copper ring let in the walls of the bore. On firing, the gas presses back the cup against the rounded end of the breech block, and thus forces the lips hard against the copper ring.

It is difficult to compare the excellence of these various systems; so much depends on the care of the gunners, and the nicety of manufacture. The German and French marine methods permit the parts to be quickly exchanged when worn, but it is necessary to cut deeply into the walls of the gun, and to make the wedge, or breech screw, considerably larger than the opening into the chamber.

The Elswick plan is decidedly better in this last respect, but it requires several hours to extract and renew the copper ring where worn.

The French land service (*De Bange*) arrangement requires no cutting into the gun, and no enlargement of the breech screw beyond the size of the chamber, while it is renewable in a few minutes, merely requiring a fresh asbestos pad when worn. As regards durability, there is probably no great difference. I have been informed that with a light gun as many as 3,000 rounds have been fired with one asbestos pad. But usually it may be considered that a renewal will be required of the wearing surfaces of any breech loader after a number of rounds, varying from six or seven hundred, with a field gun, to a hundred or a hundred and fifty with a very heavy gun. Full information is wanting on this point.

Having now decided on the material of which the gun is to be composed, and the manner in which it is to be composed, and the manner in which it is to be constructed, and having, moreover, settled the knotty point of how it is to be loaded, we come to the general principles on which a gun is designed. It must not be overlooked that a gun is a machine which has to perform a certain quantity of work of a certain definite

kind, and, like all other machines, must be formed specially for its purpose. The motive power is gunpowder, and the article to be produced is perhaps a hole in an armor plate, perhaps a breach in a concealed escarp, or perhaps destructive effect on troops. These articles are quite distinct, and though all guns are capable of producing them all to some extent, no gun is capable of producing more than one in the highest state of excellence.

Thus, for armor-piercing, a long pointed bolt, nearly solid, is required. It must strike with great velocity, and must therefore be propelled by a very large charge of powder. Hence an armor-piercing gun should have a large chamber and a comparatively small bore of great length.

For breaching fortifications, on the other hand, curved fire is necessary; the escarps of modern fortresses are usually covered from view by screens of earth or masonry in front, so that the projectiles must pass over the crest of the screen, and drop sufficiently to strike the wall about half-way down, that is to say, at an angle of 15° to 20°. To destroy the wall, shell containing large bursting charges of powder are found to be particularly well adapted. Now it is clear that, for a shell to drop at an angle of 15° or 20° at the end of a moderate range, the velocity at starting must be low. Hence, for pieces intended for breaching no enlarged powder chamber is wanted; the effect on the wall is due to the shell, which must be made of a shape to hold the most powder for a given weight; and, therefore, rather short and thick. This gives us a large bore, which need not be long, as little velocity is required.

For producing destructive effect among troops, a third kind of projectile is employed. It is called shrapnel, and it consists of a thin shell, holding a little powder and a large quantity of bullets. The powder is ignited by a fuse, which is set to act during flight, or on graze, when the shell is nearing the object. The explosion bursts the shell open, and liberates the bullets, which fly forward, actuated by the velocity of the shell at the moment of bursting. Hence, to render the bullets effective, a considerable remaining velocity is requisite. The gun must therefore take a large powder charge, while, as the shell has to hold as many bullets as possible, the bore must be large enough to take a short projectile of the given weight. Thus the proportions of the shrapnel gun will be intermediate between those of the armor-piercing gun and the shell gun.

There are certain axioms known from experience, which should be mentioned here. First, the length of the powder chamber should not be more than about $3\frac{1}{2}$ or 4 times its diameter, if it can possibly be avoided, because, with longer charges, the inflamed powder gas is apt to acquire rapid motion, and to set up violent local pressures. Next, the strength of a heavy gun, as reckoned on the principle of all the metal being sound and well in bearing, should not be less than about four times the strain expected.

Again, though there are several opinions as to the best weight of shot for armor piercing, in proportion to diameter, yet amongst the most advanced gun makers, there is a growing tendency towards increased weight. The value of $\frac{w}{d^3}$, that is, the weight in pounds divided by the cube of the diameter in inches, as this question is termed, is in the hands of the Ordnance Committee, and it is to be confidently hoped that efforts will shortly be made to arrive at a solution. In the mean time, from about .45 to .5 appears to be a fairly satisfactory value, and is adopted for the present.

Lastly, it may be broadly stated, that with suitable powders, a charge of one-third the weight of the shot demands, for most profitable use, a length of bore equal to about 26 calibers; a charge equal to half the weight of the shot should be accommodated with a bore of about 30 calibers; while a charge of two-thirds the weight of the shot will be best suited by a bore 35 calibers long. Of course, in each case, greater length of bore will give increased velocity, but it will be gained at the expense of additional weight, which can be better utilized elsewhere in the gun.

The amount of work performed by gunpowder, when exploded in a gun, is a subject which has engaged a vast quantity of attention, and some highly

ingenious methods of calculating it have been put forward. Owing, however, to the impossibility of ascertaining how fast the combustion of large grains and prisms proceeds, a very considerable amount of experience is required to enable the gun maker to apply the necessary corrections to these calculations; but, on the whole, it may be said that, with a given charge and weight of shot, the muzzle velocity may now be predicted with some accuracy.

You now have the chief data on which the designer bases his proposals, and lays down the dimensions of the gun to suit such conditions as it may be required to fulfill. In actual practice, the conditions are always complicated, either by necessities of mounting in particular places, such as turrets and casemates; or by the advantages attending the interchangeability of stores, or other circumstances; and it requires great watchfulness to keep abreast of the ever growing improvements of the day.

I will now conclude with a few words on the power of the heavy guns, when employed in various ways. The first consideration is accuracy of fire. No matter how deadly the projectile may be, it is useless if it does but waste itself on air. Accuraccy is of two kinds—true direction and precision of range. All modern guns are capable of being made to shoot straight; but their precision of range depends partly on the successful designing of the gun and ammunition, so as to give uniform velocities, and partly on the flatness of the trajectory. The greater the velocity, the lower the trajectory, and the greater the chance of striking the target. Supposing a heavy gun to be mounted and in the fortresses round our coasts, and aimed with due care, the distance of the object being approximately known, we may fairly expect to strike a target of the size of an ordinary door about every other shot, at a range of a mile and a half. Here we have carriages mounted on accurately-leveled platforms; we have men working electric position finders, and the gunners live on the spot, aud know the look of the sea and land round about.

Now, consider the case of guns mounted in ships. You at once perceive the difficulties of the shooter. Even supposing the ship to be one of our magnificent ironclads, solid, steady, yielding little to the motion of the water, yet she is under steam, the aim of her guns is altered every moment, some oscillation is unavoidable, and she can only estimate the range of her adversary. Great skill is required, and not only required, I am glad to say, but ready to hand, on the part of the seamen gunners; and low trajectory guns must be provided to aid their skill.

If we go to unarmored ships of great tonnage and speed, we shall find these difficulties intensified; and if we pass on to the little gunboats, advocated in some quarters for attacking iron-clads in a swarm, we shall find that unsteadiness of platform in a sea-way renders them a helpless and harmless mark for the comparatively accurate practice of their solitary but stately foe.

The destructive power of guns is little known to the general public, and many wild statements are sometimes put forward. Guns and plates have fought their battle with varying success for many years. One day the plate resists, another day the gun drives its bolt through. But it is frequently overlooked that the victory of a plate is a complete victory. If the shot does not get through it does practically nothing. On the other hand, the victory of the gun is but a partial triumph; it is confined to a small arc. I mean, that, when the plate is struck at an angle exceeding 30° or so, the shot glances harmlessly off; while, even when perforation is obtained, it is at the expense of the more deadly qualities of the projectile, which must be a nearly solid bolt, unable to carry in with its heavy bursting charges of powder, or destructive masses of balls.

About six years ago, an experiment carried out at Shoeburyness taught a lesson which seems to be in danger of being forgotten. We hear sometimes that unarmored vessels are a match for ironclads and forts; and I will conclude this paper with a short extract from the official account of the results of firing shrapnel shell at an unprotected ship's side. I shall say nothing of boilers and magazines, but shall state simply the damage to guns and gunners.

A target was built representing the side of a certain class of unarmored

ships of war; behind this target, as on a deck, were placed some unserviceable guns, mounted on old carriages, and surrounded by wooden dummies, to represent men working the guns. The attacking gun was a 12-ton 9-inch muzzle-loader, of the old despised type, and the projectiles were shrapnel shell. The charges were reduced to represent the striking force at a range of 500 yards. Two rounds did the following damage inside, besides tearing and ripping the ship's side in all directions.

1st Gun.—Seven men of detachment killed.

2d Gun.—Carriage destroyed; six men blown to pieces, all the remainder of the detachment severely hit.

3d Gun.—No damage to gun or carriage. Five men killed, one blown to bits, and one wounded in leg.

4th Gun.—Gun dismounted. The whole of the gun detachment blown to pieces.

That is the amount of destruction achieved in an unarmored ship by two rounds of shrapnel shell.

I beg to thank you sincerely for the kind attention you have given to me this evening.

DISCUSSION.

Gen. Sir JOHN ADYE, K.C.B. (Surveyor-General of Ordnance), said he should like to refer for a few moments, retrospectively, to the great development of artillery science, which commenced 25 years ago, and which, even now, hardly showed signs of having reached finality. England was amongst the first to use rifled ordnance, and at the outset the breech-loading system was adopted, the result being that we obtained guns with range and power far beyond anything known before. The system was consequently adopted with enthusiasm, and between 1858 and 1862, an expenditure of 2½ millions sterling was incurred in the manufacture of armaments for our fleet and fortresses. But although these guns had many excellent qualities, they soon developed serious defects; they were found to be delicate, liable to get out of order at critical moments, and accidents occurred to the men who served them. The result was, that a decided change of opinion took place; the officers and men, to some extent, lost confidence in their weapons, and were as anxious to get rid of them as they had been to obtain them. This led to the Government instituting careful inquiries; there were the Armstrong and Whitworth competition in 1863, various trials in 1864 and 1865, and committees appointed between 1865 and 1870, who all pronounced a unanimous opinion that a muzzle-loading system ought to be introduced. In consequence of this, for several years all guns up to 80 or 100 tons were made on that system. The Germans, on the other hand, adhered to the breech loading system throughout, though in their early experience they met with many difficulties as we have done, and in the great Franco-German war, many of their field guns, though firing small charges, broke down, whilst the siege guns before Paris were weaker still. Indeed, Herr Krupp himself wrote to the *Times* in 1878, saying that after that war the German Government remodeled the whole of its field and siege breech-loading artillery. Both the War Department and the Admiralty in 1870, and again in 1875, expressed their confidence in our armaments, both as regarded power, range, and accuracy. In 1874 and 1875, in fact, England held a leading position in respect to artillery. At that time, no doubt, many people hoped we had approached finality; but their hopes were disappointed, for about that time another great stride was taken in artillery science, the full effects of which had not yet been realized. This great progress was due very much to the researches of the Explosive Committee, and partly to the experiments of other nations. That committee had been sitting from 1868 to the present day, and had gradually discovered that guns could be made of far greater power, range, and velocity than those manufactured seven or eight years ago; indeed, speaking roughly, he might say that the guns of to-day, whether muzzle or breech loading, were nearly double the power of those of the same weight made eight years ago. Some people were under the impression that a muzzle-loading gun was, in itself, weaker than a breech loader, but that was not so. Guns could be made on either system, practically identical in weight and power; rapidity of fire, he believed, was

rather on the side of the muzzle loader. But it was found that these new types of guns required to be made of immense length and with large chambers, firing heavy charges, and in such cases there was an evident advantage in loading at the breech. At the same time it is hoped that the introduction of improved slow-burning powder will prevent the distressing accidents which sometimes occurred in early days. It was quite right, under such circumstances, that Government should institute careful experiments, with a view to the introduction of heavy breech loaders; but the progress of re armament could not be very rapid. It was a work of time and expense, and he believed, also, that new ships would be required for the very large guns of the new type, so that re-armament must be gradual. With regard to the introduction of steel, he thought caution should be used; he could quote many accidents which had occurred to guns made entirely of steel, in particular two which had happened to Krupp's guns, one in Constantinople and another recently on board a German ship. So far as he knew, the wire gun, which had been under experiment by Sir William Armstrong, had given very remarkable results, and might possibly lead to a satisfactory solution of this matter. The Government had recently established an Ordnance Committee, for the purpose of considering the scientific questions involved in all these matters, consisting of distinguished officers of the artillery and Navy, and of two eminent civil engineers, one of whom he was pleased to see in the chair. This committee had a large accumulation of knowledge at their disposal, gathered both at home and abroad, and their was reason to hope that their reports would give confidence to the public. The Admiralty and the War Department were in complete accord on these matters, and the committee would be glad to receive assistance from civil engineers or others who had practical knowledge. There was a determination on the part of the Government that, in the future, as in the past, England should hold a leading position as to its armaments, and be prepared to guard the interests of the Empire in every part of the world.

Professor ABEL said that though he had witnessed the development of rifled artillery from its earliest stages, and also the successive battles between breech and muzzle loaders, he should not have felt competent to speak on this subject, but for the fact that he was somewhat intimately acquainted with the propelling agent employed in guns, and also with the material used in their construction. In reference to what Colonel Maitland had said in his paper with regard to the preference shown by England for wrought iron, its employment was then a matter of necessity, and not of choice, because when rifled guns were first adopted, though we knew almost as much as we do now about the working of malleable iron in large masses, the knowledge concerning the production and treatment of large masses of steel was very limited. In fact, practically almost the only person who had successfully dealt with the production of cast steel in large masses was Krupp, of Essen, and even in his hands it was still a child, and the material he then produced was confessedly often unreliable. The English Government, advised by one of the most distinguished engineers of the country at that time, constructed rapidly and readily guns of wrought iron of considerable size, and which were, at that time, as superior in point of uniformity to cast steel guns as they were in point of strength to cast iron. Since then, we had learned much as to the properties of steel and how to deal with it; and, thanks to the development of the Bessemer and kindred processes, we could produce steel of high and uniform quality, and of the requisite softness and temper; and no one could now doubt that steel competed successfully with wrought iron as a material for ordnance. But even now, if it had not been for the improvements effected in the manufacture of gunpowder, steel would probably not be so applicable as it was for ordnance, since, even in its best form, it was, he believed, not so capable of resisting the sudden strains inflicted by violent powder as malleable iron; Colnel Maitland had told them that long after it was used abroad, it was found necessary to strengthen the breeches of steel guns by envelopes of wrought iron. The present improvements in gunpowder had originated in the researches to meet the requirements of the earlier rifled

guns, carried out by the earliest Explosive Committee, of which he was a member; and the broad principle was laid down, which had up to the present been adhered to, that the proportions of the powder constitutents having been fixed upon as being calculated to give the maximum development of force for a given weight of material, it was advisable, if possible, to moderate its action, not by altering its composition, but by modifying its physical and mechanical characteristics. The improvements alluded to by Colonel Maitland had been entirely of that nature; great attention had been paid to means for regulating its density and hardness, and the size of the individual masses, and to other minor points necessary to secure uniformity, a result the attainment of which was by no means so easy as might be supposed. By these means they had succeeded, at first very gradually, but lately more rapidly, in moderating the rapidity of action of gunpowder, until it became comparatively manageable. But even these improvements introduced difficulties which it required all the skill of the manufacturer and the chemist to overcome. Amongst the difficulties incident to the use of large charges, was the erosion of the surface of the bore; and although this was partly overcome by means of bands or discs of soft metal, the erosion of guns was still a serious question which was taxing all their energies and skill to meet. Colonel Maitland had given some interesting views as to the effect of the retardation of the shot, and the gradual development of the force of the powder in large charges. The idea of retardation of the shot, referred to as a novelty, was mooted many years ago, and several plans were brought forward to effect it. With one or two exceptions, they were not very practical, and their object was scarcely the same as that aimed at by Colonel Maitland. But it did appear that the principle of retardation was applied in the earlier rifled service breech-loading guns, which fired lead-coated projectiles. The forcing of the soft metal coating into the rifled grooves had a retarding effect on the projection of the shot, and it was this very fact that led to the disovery of the necessity for moderating the rapidity of explosion of the powder then used.

Colonel Hope, V.C., wished to correct a slight misapprehension on the part of Colonel Maitland, with regard to Mr. Longridge having abandoned his steel-wire principle. From a letter received from him only yesterday, it appeared that he was still convinced that his principle was correct, and he was only afraid that Sir William Armstrong would fail because he was diverging somewhat from it. He had listened with great pleasure to the remarks of Sir John Adye. It was quite true, as he said, that at one time England was ahead of all other countries in heavy ordnance; but, nevertheless, he thought then, as he did still, that those immense guns were constructed on wholly erroneous principles; and that seemed to be now acknowledged, because those guns were already obsolete. With regard to the gunpowder, the composition was the same, and if there were any difference in the strength, the large sized powder was the weaker. You had only to stand behind one of these now obsolete guns when fired, and you saw grains of powder passing through the air outside the gun, showing that the whole of the powder was not converted into gas inside, and of course, any portion not so converted was absolutely wasted. The only gain obtained by increasing the size of the grains was that you humored the weakness of the gun, by making the combustion slower. If the gun were strong enough, the more violent the powder, giving the more complete conversion into gas, the better the result, as you got something of the effect of a blow as well as a push. If he mistook not, the powers of the Ordnance Committee were limited to certain matters which were referred to them; and therefore it was not of the nature of a scientific committee, able to undertake original research, and it did not at all follow that any system which might be ultimately approved would be the best, or the one which they would themselves have approved, had their powers been larger. He had heard with great pleasure the statement that the Government would welcome assistance from all comers, which was a complete change of affairs within one or two years. Theoretically, Government was always ready to receive assistance from anybody, but it was under conditions

that few people would be willing to accede to. The consequence was that if any one were foolish enough to offer to assist the Government, they insisted on his surrendering all his secrets, and assisting in every possible manner, receiving only such reward as the Government Department itself should fix. He never could see why guns, ships, and kindred matters should be treated in a different way to every other question of a similar nature. If the War Office required an acre of land to increase an existing fortification, it could take it, but it had to pay a fair price, to be fixed by arbitration; and why should it arrogate to itself the right to sieze a man's invention, and use it, and pay him whatever crumb some one in authority chose to fling to him? He had just had the misfortune to lose, by death, one of his earliest friends, Sir William Palliser, a man who had made many inventions, some of which were taken up by the Government, and some were not. One of these was the converting of old smooth-bore cast iron guns into rifled ordnance. About 2500 of these guns were in constant use, and down to a very recent period, at any rate, not one of them had ever burst, or caused a serious accident; and he thought it was a great pity that this system had not been applied to larger guns, such as the one shown on the drawing. This principle was to line a cast-iron gun with a wrought-iron tube, and he thought it a pity that so cheap and easy a system had not been adopted instead of the more costly and cumbrous one which had already broken down. There was no doubt that foreign countries were ahead of us in the use of steel, with the single exception of this system of steel wire, which he did not profess to understand. Some specimens of guns he had seen on the Continent were altogether in advance of anything he had seen at home, and a gun, 18 feet long, was now being made for him on the Continent, of cast steel tempered in oil, which was altogether superior as regards metallurgy to anything he had seen in England.

Professor ABEL said he had endeavored to explain that the composition of powder had been in no way changed, and the total force developed. With regard to burning particles being seen to come out of the gun, he believed the same thing occurred with the quickest and most violent powders used; but it was only a very small proportion of the charge. It was scarcely the correct view of the cause of modifications effected in gunpowder to say that they had been introduced to humor the weakness of structure of the guns; the real object and effect were to develop the full power of those guns. Artillerists now obtained velocities never dreamed of before, and with much less maximum pressure on the gun.

Major CHARLES JONES said it was quite true that there were a large number of the Palliser guns in the service, but they were all of a moderate size, the charge of the largest being 10 lbs. He believed the system had been recently applied in America to larger guns, but not very successfully, two having burst very recently. With regard to an observation of Colonel Maitland's, on the inaccuracy of fire from gunboats, he thought he had gone further than the gentleman he referred to intended. The vessels he advocated were cruisers, ranging from 2000 to 3000 tons burden, and they could scarcely be regarded as small boats, bobbing about on the surface of the ocean; in fact, the accuracy of their fire would be approximately as great as that of an ironclad. The argument was, that you would not have only one, but several cruisers fighting an ironclad, carrying an equal number of guns, moving with greater rapidity, choosing their own range, and concentrating their fire, whilst the armor-clad vessel would have to disperse her fire and aim at a much more rapidly moving object. There could be no question about the damage done to an unarmored vessel by heavy ordnance, but the damage done to an ironclad, although fewer shots got through, was not quite so small as the remarks made might lead one to suppose. When you got a Palliser shell, or even a Palliser shot, through, those projectiles themselves broke up, and not only so, but they carried with them pieces of the plate and backing, and bolts through in a shower, and the effect would be practically the same as the bursting of a shell inside an unarmored ship. As regarded the ribbon gun, two were made, to begin with, and the first one, made in 1879, had stood very large charges and

showed no signs of failure; that was a six inch, but the one now under experiment was 10.263 inch. One reason why guns had grown in length was the advantage which had been found to arise from air spacing, firing the charge in a chamber not completely filled. That result was deduced from the experiments of Prof. Abel and Captain Noble, showing that pressure in a closed vessel was due to the density of the charge; as the density increased, so did the maximum pressure, and it naturally followed that by giving the charge in the gun less density, not filling the chamber, you were able to keep down the maximum pressure; but in order to get the full effect of the powder, it was necessary to lengthen the bore; and that, again, led to the adoption of enlarged chambers. That was begun some years ago, by Captain Noble, in order to give airspace, without unnecessarily encroaching on the bore.

The Chairman said that the more he considered the question of artillery, to which his attention had been directed for several years before he joined the Ordnance Committee, the more he was struck with the difficulties that surrounded the subject. A weapon was required which should not only be a source of danger to the enemy, but should also be so absolutely safe as to inspire the utmost confidence in those who used it. As a civil engineer, he was accustomed to deal with large pressures, as they were considered, of elastic vapors. Steam was now being used at a pressure of 1,000 lbs. to the square inch, which was thought very great, and great precautions were taken against accident. But this pressure sank into insignificance when compared to that in the chamber of a large gun, which was very commonly 17 tons to the inch, or 38,000 lbs., and sometimes went up to 25 and even to 30 tons. The problem was, to make a vessel which should bear this enormous pressure, without exceeding the elastic limit of the material. They all knew now that which was not so well known formerly, that if the bore of the gun were surrounded by a mere block of metal, with all the parts in a quiescent condition, without any initial strain, the pressure which came upon the interior of that block had to be resisted by the immediately neighboring metal, without that metal receiving any substantial aid from the metal further removed from the center; in that way the interior metal yielded, and was fissured, and when once this occurred the fissure increased until the whole gun was rendered unserviceable, or, perhaps, burst explosively. It was, therefore, found impossible in gun making to deal with metal in the ordinary condition in which an engineer was content to deal with it, and therefore recourse had been had to that which was known many years ago as a mode of obtaining the aid of the whole of the metal in the construction, when pressures such as obtained in the Bramah presses were employed; and it was on that principle that all modern guns were made. With respect to the wire or ribbon gun, while following the example set by the reader of the paper, and not attempting to prophesy until the event had happened, he could not help thinking that it would be well to investigate this subject thoroughly. Whether it would succeed or not he would not say, but the experiments which had been made were favorable. There was great simplicity of construction, and great certainty in some respects. You had not to deal with those difficulties which arose when metal was shrunk on at a high temperature, though he must admit that the great practice and skill in the gun factories, and the great knowledge of their superintendents, enabled this to be done with nearly absolute certainty. But when dealing with material in the cold state, as in the easily-manageable form of a ribbon, it was obvious that you could control the tension put upon it, you could wind it on as you wished, and if you did not like it, you could undo it. Again, you could not get any serious local latent defect, such as it was impossible always to avoid in a large mass of metal. If in the ribbon there were defects, they would only be local; and where you had twenty coils, one over another, surrounding a ten-inch gun, it was not likely that the defects would be cruel enough to occur in the same radial line in the whole twenty convolutions, or indeed that more than one of them would thus occur. Probably nothing but the most severe test would tell whether that construction which appeared so substantial, when first

made, would be substantial after the concussions arising from repeated discharges; but he thought one might be very hopeful about it. He had been surprised to hear one observation from Professor Abel, which, in any view, he was sorry for. He should be very sorry to suppose that Professor Abel was wrong, but still more sorry if he were right. He referred to his observation that steel was less competent than wrought iron to resist a sudden action upon it. There was steel and steel, and if the observation had been made by some one who was not acquainted with the subject, and when thinking of steel had present to his mind a file, which would break in two if dropped on a stone floor, he should not have paid any attention to it; but, coming from a gentleman of Professor Abel's experience as a chemist and a metallurgist, he was surprised to hear it. He should certainly have said that steel was more able even than wrought iron to withstand percussive action. But he thought it was clear that, whether in the form of wire, or of hoops, or of forged pieces, owing to the changes in the powder and in other ways alluded to by Colonel Maitland, the large guns of the future would probably be made of steel, a metal which certainly would give a strength not to be obtained from wrought iron. Reverting again to the ribbon gun, he might remark that in a gun ordinarily weighing 25 tons, one might look in the ribbon mode of construction for a saving in weight of four to five tons, or if the weight were the same, to a proportional increase in strength. A question had been raised as to the longitudinal strength of ribbon guns, and reference had been made to three modes generally adopted for insuring it—the French, Sir William Armstrong's, and the mode adopted at Woolwich; probably any of these would suffice, but the French appeared to be cumbrous, and if the drawings he had seen were accurate, he feared there might be a failure, not on account of the material, but because there were sudden changes of form which must lead to rupture. This, however, was a mere matter of construction, which could be easily altered, and might be only an error of the draughtsman who prepared the drawings he had seen.

Colonel MAITLAND, in reply, said he should not like any one to think he had omitted Mr. Fraser's name from any personal feeling. He worked with him every day, and he thought it would be almost like self-praise to mention his name particularly. With regard to Mr. Mallet, Tubal Cain, and others, he had not gone back so far. It was said that retardation for regulating the consumption of powder was old, but it was so new that until a few days ago it was not believed in. Like many other discoveries, people said at first there was nothing in it, and then they said they knew it all the time. In breech loaders, until very recently, they had always had large air spaces in order to mitigate the violent effects of the powder. But the air space was waste; if you filled it with powder it was more useful, and if you could hold the shot back so as to consume a slower powder at a steady pressure you could use a much larger charge. The other day he fired a 10.4-inch gun with 231 lbs. of powder and a large air space on the old principle, and the velocity obtained was about 1,950 feet. He then fired it with a very large charge of slow powder and strong band, and got 2,150 feet. With regard to the gunboats and cruisers, Major Jones seemed to know whom he alluded to, but the views he referred to were put forward by several others; and his special argument was against small gunboats, which were supposed to come out in swarms to attack an ironclad. He believed that half of them would be sunk. He heard an interesting lecture lately on the subject Major Jones alluded to, that of attacking an ironclad like the *Inflexible* by three large cruisers. An eminent engineer, he met coming out, said it was a very interesting subject, and he asked him which he would rather be in, the armored or the unarmored ships. His reply was, that in the present state of science he should prefer the ironclad ships, and he quite agreed with him. The principle of enlarging the chamber of guns, so as to hold a larger charge of powder, was thoroughly worked out in 1873 in the Royal Gun Factory; Major Jones appeared to think it was due to Captain Noble. (Major Jones disclaimed this, and explained that he referred only to the use made of enlarged chambers for air spacing.) Colonel Maitland said that in that case he had nothing to add, except to offer thanks for the kind attention given to him.

ON TORPEDO BOATS AND LIGHT YACHTS FOR HIGH SPEED STEAM NAVIGATION.

By JOHN ISAAC THORNYCROFT, M. Inst. C.E.

From Proceedings of the Institution of Civil Engineers.

II.

Mr. W. H. WHITE, Assistant Constructor of the Navy, desired to allude to the bearing which the author's system of propulsion might have upon steam navigation at higher speeds than those generally obtained; but before referring to that subject he wished to draw attention to one or two points in the paper from a naval architect's point of view. The author had discussed the matter chiefly as an engineer. Mr. White would speak of it as a naval architect who might have to put engines of the torpedo-boat type into ships; and would endeavor to make clear what seemed to his mind the obvious limitations under which, in the present condition of things, the system could be extended. The torpedo boats, which he thoroughly admired as triumphs of engineering and constructive skill, were exceptional in almost every respect in which the naval architect could view them. There was extreme lightness of construction, extreme lightness of machinery, and (which the author had not dwelt upon with the force with which the naval architect would dwell upon it) an extremely small load carried. The author had stated that the weight of the hull in the torpedo boat, taken as a fraction of the displacement, compared exceedingly well with the weights of the hulls in large vessels. Of course, he was aware that while it was an interesting thing to express the weight of the hull as a fraction of the displacement, that comparison could only be applied fairly between vessels somewhat similar in structure and not very dissimilar in size. Speaking generally, he thought it might be said that in the torpedo boats the limit of lightness in one direction had been reached, viz., in regard to local strength and durability; unless some different and stronger material to build with could be found. He did not think the skins could be made thinner than $\frac{1}{16}$ inch, and that thickness did not admit of a very long wear. It appeared from Table II that the weight of the hulls in these boats was one-third of the displacement, and that ratio worked out to about the same in the "Devastation" and in the "Iris," the steel-built dispatch vessel. It should be borne in mind, however, that the torpedo boat was not nearly so elaborately fitted as a ship built for self-sustaining and sea-going service. The hull was a shell to carry powerful machinery and a light load of ornament; and the limit of the weight of the hull was fixed by considerations of local strength and durability. If torpedo boats were compared not with such large vessels as those mentioned in Table II, but with smaller boats, boats intended to bear the rough usage of actual service, such as large steamboats in the navy, the torpedo boat would not come out on the same line, but would be vastly lighter. The hull would probably weigh about half the displacement of a boat built in the ordinary way, whereas the author could build a satisfactory hull with one-third. He should be glad if the author would state in his reply if the percentage for weight of hull in the second-class boat was smaller than in the first class. With regard to the question of load, Mr. Yarrow had stated that, in a first-class boat, the addition of ten tons to the load would take $2\frac{1}{2}$ knots off the speed. According to Table IV., the same boat (first-class No. 10) when tried with a small addition to the load, at once lost sensibly in speed. There had been cases in the navy in which boats when fitted up with a full torpedo armament had been tried, as compared with trials before they were so fitted, and it had always been found that there was a falling off in speed as the load was increased. He should not be doing injustice to the author's first-class boat in saying that, in doubling the load she had to carry, excluding the engines, but including coals, an addition would be

made of from 40 to 50 per cent. to her displacement. That was an enormous addition to have to make for adding, say, 6 or 7 tons to the weight carried. In these designs, the shipbuilders were running on narrow and critical lines; and no one could study the matter as an outsider, without feeling that the author and Mr. Yarrow had been doing wonders in the limits within which they had to work. He had no desire to criticise, but merely wished to illustrate the fact that in torpedo boats there were extremely exceptional conditions. He agreed with Mr. Wright that for short passages, such as those of the channel service, it might be better to use the uneconomical engines (uneconomical as to fuel, but wonderfully economical as to weight) instead of the compound engines. In the fast vessels recently built by Mr. Samuda, M. Inst. C.E., simple engines had been used by preference for the Folkestone and Boulogne service, because of their relative lightness as compared with compound engines, accepting their more wasteful conditions as to fuel. But while this might be true for short passages, it did not hold for long voyages. For example, on the Transatlantic service, any one looking into the figures would see that, taking the coals and the engine together, there would really be a heavier combination to use those very light engines which burned practically twice as much coal, than there would be under existing conditions with compound engines. And as to the Australian service, the torpedo type of engines, if they could run all the way, could not be put into the ships now working on that service because of their high rate of coal consumption. The limits of employment of that type of engine under the present conditions of economy in fuel were very narrow, and it did not seem likely that the locomotive type of boiler, and the light, quick running machinery could be substituted for the present compound engines, until great changes had been made in their relative economy of fuel. He regarded the trials conducted by the author and Mr. Yarrow as model experiments in steam propulsion. Applying to those trials the law of "corresponding" speeds first indicated by Mr. Reech, and since worked out and applied to model experiments by the late Mr. Froude, imagination might extend from those tiny little vessels up to vessels of any size and any speed. Engineers might, perhaps, overpass the mark in some of these speculations as to the future; but, from the steam trials of torpedo boats, they could see with greater certainty what lay before them when they came to try and increase the speed of large ships. He had been endeavoring to see his way in this direction, and would give a brief sketch of the conclusions at which he had arrived. When a torpedo boat was under way, it appeared to be going almost in smooth water, except for a curious hump of water appearing here and there, along the length of the boat. He remembered a gentleman, when speaking on a paper by the late Mr. Froude, saying that the waves which were said to travel with the torpedo boats were imaginary, because the boats were practically in smooth water. That statement simply meant that the principal series of waves accompanying the torpedo boats were so large as compared with the boats that to a person on board they were unobservable: owing to their magnitude, they were beyond the reach of his observation. But when models of the same torpedo boats were tested in the manner proposed by Mr. Froude, all the beautiful wave phenomena which accompanied propulsion could be mapped out in detail, as had been done in a paper recently written by Mr. R. E. Froude. He had arrived at the interesting conclusion that, as a torpedo boat starting from a low speed was driven gradually to higher speeds, she passed through what might be called two phases. If, for example, she was driven at a speed under ten knots, she behaved as ordinary vessels would behave at ordinary speeds, say up to 14 or 15 knots. The resistance varied as the square of the speed; the vessel changed trim a little by the bow, and sank a little deeper in the water. That conclusion from the model had been verified by the author in actual trial, and the result would be found in the Reports of the British Association. As the speeds increased, the torpedo boat passed on, still like an ordinary ship, to a condition where the resistance varied at a greater rate than the square of the speed; in fact, he believed that in some cases it reached the $3\frac{1}{2}$ power of the speed. But as the speed was still further increased, the boat passed through a

maximum rate of variation of the resistance in terms of the speed, and then it began to drop; so that the same boat which at 10 knots had a resistance varying as the square of the speed, and at 13 knots reached the $3\frac{1}{2}$ power, at 18 knots had a resistance varying as the $1\frac{1}{2}$ power of the speed; and beyond that the resistance diagram became a straight line, the power varying directly as the speed. The difficulty was how to get ordinary vessels to the speed where they behaved in that way. The author had attained this speed in his boats, and Mr. Yarrow also, but he must have been surprised the first time that result wa sobtained. Looking back at the trials with the "Miranda," no one could have been more surprised than the author at the result obtained at the higher speeds. When that curious change in the law of resistance took place, the boat which had been settling down a small amount, and going deeper into the water, behaved in that way no longer. She altered her trim, the bow seemed to come out of the water, and there was a bodily rise, so that if the whole situation could be frozen, there would be found less water displaced in weight than the weight of the boat itself. That change of trim most people had been inclined to believe was due, not to the wave slope, but to 'some variation in the fluid pressures along the bottom of the boat. It had now been proved by Mr. Yarrow from the boats themselves, and by Mr. Froude by means of his model experiments, that that was not entirely so. Mr. Yarrow had shown by actual measurement that the water surface alongside the boat, when the bow was apparently out of water, was nearly the same as in the still water: in other words, the boat was riding on the back slope of a wave which she had created by means of her machinery. When he was once in company with the author in a boat on the Thames, they seemed to be traveling in comparatively smooth water, but they suddenly met one of the saloon steamers, which thereupon danced about as if she had been a big cork, simply by the waves which the torpedo boat was causing, and which they had scarcely noticed until they had that means of observation. As to the speed at which similar phenomena might be expected to occur in large-sized ships, when the power would vary as the speed, a few remarks might be added. A comparison of form had been made in the paper between H. M.S. "Iris" and the first-class torpedo boats, and he would carry the comparison a little further. What would be the speed in a vessel of the size of the "Iris," —a torpedo boat enlarged—at which that condition of economical propulsion could be reached? He need not go into details, but the speed would be from 35 to 40 knots an hour in a vessel of 3,700 tons. At that rate the power would increase directly as the speed. The difficulty was how to attain that point. The ship would then be experiencing a resistance about $\frac{1}{20}$ of her displacement, and she would require, by the analogy of the torpedo boats, an engine power, to put it modestly, of 45,000 indicated HP. to drive 3,700 tons through the water at the speed which was economical when looked at in a certain way. He believed that a company had been formed to cross the Atlantic at a uniform speed of 20 knots. He had no wish to criticise the prospectus of that company; but, as a matter of fact, 20 knots in a ship of the size of the first-class ocean steamers would correspond to very low speeds in torpedo boats—to speeds in which they behaved just like ordinary vessels. Taking an Atlantic steamer of 10,000 tons displacement, he found that to get an economical propulsion such as was obtained at 18 knots in the torpedo boats, a speed of from 45 to 50 knots would be required. He had not thought it necessary in this case to work out the HP. He was not an engineer, and he would leave the author to say how 40,000 HP. could be usefully employed on a vessel of limited draught, what sort of propellers would be required, how the machinery was to be stowed in the ship, and how the power of traversing great distances at sea could be obtained under the assumed conditions. He did not think it could be done by simply pushing the locomotive system, which was so admirably carried out in the torpedo boats, to the larger scale that would be required. As far as he could see, if the existing "Iris" could have 45,000 HP. put into her hold, in the form of machinery resembling that used in torpedo boats, and equally light in proportion to the power developed, her coal supply would be reduced to 550 or 600 tons (instead of

750 tons as at present), and her rate of coal consumption would be so increased that, at full speed, she would only be able to run for about six or seven hours before her coal was exhausted. At all lower speeds, such as commonly occurred in practice, her coal consumption would also be very much increased by the adoption of the torpedo-boat type of machinery. He had the greatest admiration and respect for the work done by the author and by Mr. Yarrow; but, looking at the matter all round, he could not help thinking they were still left somewhat in the dark as to how similar operations could be carried out on a large scale.

Mr. NATHANIEL BARNABY, C.B., Director of Naval Construction, could confirm the statement, as he feared all gray-bearded naval architects would have to do with some sorrow, that ten years ago they would have said that was impossible which the author had effected, and as he had achieved the impossible it might be well hoped that the impossible might be achieved again, if not by himself, by some one else. Mr. White had fairly stated the difficulties lying in the way of any attempts to get high speeds out of ordinary ships. He was speaking before gentlemen who were familiar with all the forces of nature used in the service of man, and he therefore spoke in the expectation that there might be some present who would set before themselves the problem how to get the enormous but necessary powers of which they had heard into ships of moderate size. He might mention as some encouragement that there was certainly one gentleman, Mr. Ramus, a clergyman, who thought that he had found a way of doing it. He had had some experiments made by Mr. Froude. His view was that it was possible to get high speeds with ships of a certain form which he proposed. He did not use the screw at all, but a rocket force to propel the ship. He was much disappointed when Mr. Froude's experiments led him to say that the result could not be accomplished in the manner proposed.

Referring to torpedo boats of high speed, another type, that of "Herreshoff," had been spoken of as a failure, but that was not a correct term to apply to it. It was a very clever boat, not an imitation of the author's, or of Mr. Yarrow's proposal, but a genuine American production. At least one boat builder in England had taken advantage of it, and produced an excellent arrangement by which he got a high speed going astern as well as going ahead. Mr. Yarrow had had some disappointment in regard to the speed going ahead, and he stated that he placed the screw under the keel of the boat as Mr. Herreshoff did. He had never asked the question, but it would be obviously necessary to have a different speed of engine, or a different pitch of screw, or both, in order to get as good a result when working in water which had a much higher speed with regard to the boat than the water which was at the stern. There might have been some difficulty in regard to the adjustment of the engine or the screw which had not yet been fathomed. At any rate, the results obtained by the Herreshoff boat were exceedingly good. The difficulty arose from the coiled tube boiler. In his view the boat was in no sense a failure, and he thought that in a year or two it would be acknowledged that England had gained a great deal by the experience which that boat had furnished.

Commander W. DAWSON, R.N., remarked that when, a few years ago, he read a paragraph in a newspaper stating that the author had designed a vessel to go 20 miles an hour, he thought there must be some mistake in the figures, and when a little later it was asserted that the vessel went 20 knots an hour, he came to the conclusion that the statement must have been exaggerated. The next thing that struck him was the question, how many 20 miles would the vessel be able to do? It would be a lamentable thing for England if the base of operations for first-class torpedo boats were to be an English harbor. They should be able to go some little distance and make an attack, if necessary, on an enemy's coast. His attention had been a good deal devoted to the second-class boats. He was one of those who thought that, with all that the mechanical engineers had done for the Royal Navy, they had not altered the principles of naval warfare, and that a good deal might be learnt from the modes adopted by our forefathers. He might be permitted to mention one instance of the use of boats, occurring in

the War of Independence among the South American Republics. At the port of Callao the Spaniards had a fifty-gun frigate called the "Esmeralda," manned by about four hundred or five hundred men, protected by powerful batteries, and outside were certain booms to protect her seawards. A valiant captain, Lord Cochrane, was determined to go in small boats alongside the ship as she lay at anchor, to climb her smooth side, get on board, and carry her out of port. He discovered that there was a narrow passage between the end of the boom and the surf on the beach, and that there was room for him to carry his small boats round. He went alongside at night, climbed up the side of the frigate, cut the cable, and carried her away, the batteries firing a shotted salute as he went. That, he thought, was as desperate as any work which torpedo boats would now be called upon to do. If Lord Cochrane had had any of the author's boats, what sort of conditions would he have liked to find in them? He would have liked the boat to be as safe as possible; and as he rounded the boom one of the first things that might strike him would be the danger of the fan blade striking the boom. It might not be possible to stop the engines before all three blades went; there would then be no means of moving the vessel, and the expedition would be at an end. If the screw were done away with that danger would be obviated. Then there was a danger of the heel below the keel touching the shore in passing round the boom, in which case the boat would be thrown by the surf broadside on the beach, and thus the expedition would be terminated. By getting rid of the rudder another element of danger would be avoided. An additional source of danger would be found on getting alongside the ship from the little bolts that often projected about the water line. When an attacking boat was once within gun power, the nearer it got to the large ship the safer; but if a thin boat made of steel $\frac{1}{16}$-inch thick ran against the bolts at the side of the ship, she would be apt to get a hole in her side, and the water would come in fast. Why not make the driving engines pump out the water, and steer the boat? All those problems had to be solved. It was possible to steer a vessel without a rudder, and if there were no screw propeller, the dead wood and stern post could be cut away, thus enabling the boat to turn round more quickly. Turning upon a pivot near the bow, the boat had to displace a wall of water, and of course, the less the height and length of that wall the better. All those objects could be accomplished by a change of the mode of propulsion. In the first place, it might be necessary to sacrifice some of the speed, but he had no doubt that Lord Cochrane in his expedition would have been glad to have the elements of safety to which he had referred at the cost of 3 or 4 knots in speed. There was one other danger to which torpedo vessels were exposed. Some eminent officers connected with the Royal Navy, to whose opinion he attached considerable value, had confounded the results obtained by gunnery experiments in peace, with what resulted in war. Certain experiments have been made with machine guns at Portsmouth and Shoeburyness for the purpose of determining which gun was the best, not for the purpose of showing what war was like. Those little vessels could not exist under a fire of heavy guns, machine guns, or even of ordinary lead bullets, if war was anything like what was shown by peaceful experiments. What war was really like might be seen from an example taken from the American contest. The "Albemarle" ironclad vessel lay alongside a wharf protected by her own guns, by guns on the wharf, and by troops on shore, and she had to be attacked and destroyed. A steam vessel—not one of high speed, like the author's—provided with a clumsy kind of torpedo, went in the night alongside her and blew her up. Unfortunately, amongst the hundreds of shot fired, one touched the boat and sent it to the bottom. The dangers to be anticipated from gunnery fire, by those little boats, was very small, and not at all like what happened in peaceful experiments. In actual warfare, it took a gun's weight in shot to kill a man; so that if a gun weighed 3 tons it would take 3 tons of shot to kill one man, and if the gun was 12 tons in weight, it took at least 12 tons weight of iron shot to kill him; thus showing that, from one point of view, the act of shooting had gone backward in the interval between the ac-

tions of the "Shannon" and "Chesapeake" in 1812, and that of the "Shah" and "Huascar," in 1878. The old rule as to lead bullets was the man's weight in bullets to kill him, and probably with breech loaders and rifles, still more lead was required. The machine guns had not been tried in naval warfare, but no doubt their hitting powers would be found equally uncertain under fire, and the author need not despair as to the utility of his beautiful boats under fire, because of the apparent accuracy of machine-gun fire as exemplified in such peaceful experiments.

Mr. T. R. Crampton desired to call attention to an important point which had not been referred to. Engineers were apt, in discussions on engines and boilers, to couple everything together and call it an engine. He confessed he was at a loss to know how to form an opinion as to the value of an engine and boiler when he was told that an engine was produced which only consumed $1\frac{1}{2}$ lb. or 2 lbs. of fuel per indicated HP. That statement absolutely meant nothing, and gave no information. The author stated that his engine consumed $3\frac{1}{2}$ lbs. per indicated HP. Mr. Crampton knew something of it, and therefore it conveyed some idea to his mind; but when Mr. Marshall said that the consumption would be $1\frac{3}{4}$ lb. he had no notion what that meant. How much water per HP. was used? If he were told that, then he could form an opinion whether it was the engine or the boiler doing its duty. There were plenty of bad boilers while the engines were extremely good, the total result being bad, yet the poor engines were condemned. It would be well to have the two elements separated. The author, in the straightforward way in which he did everything, had pointed out what he had done with regard to his boiler. From a visit which Mr. Crampton had paid to Messrs. Thornycroft's establishment in its early days, he was satisfied that those results could not be produced unless the fire went up the chimney. He soon saw that the fire did go up the chimney, and that the boiler was evaporating 1 cubic foot of water with three square feet of surface per hour, and that meant reducing the weight of the boiler something like two-thirds. To get the maximum out of the boiler, three or four times the surface was required. The author said, "I cannot afford to do that, and therefore I will sacrifice fuel in order to get lightness." "I know," he said, "that the jackets are good, but in my particular case, with high speeds, I will sacrifice the advantage in order that I may get a lighter machine; my great principle is to get lightness as far as I possibly can, and economy as a secondary consideration." That kind of machinery would not therefore be expected to be put into large vessels, nor would the author himself advise it. He had, however, done a very great deal, and, indeed, had accomplished that which had been said was impossible. He remembered that when the speed of 16 miles an hour was attained he discussed the question with a member of the Institution, and they came to the conclusion that it was almost impossible with ordinary preparation. When the speed was given, he said that there must be 60 HP. to do the exhaustion, and it turned out that there was 63 HP. or 64 HP. How was that done? By letting the fire go up the chimney. No one whom he had ever met had had so great an appreciation of the reduction of the weight to a minimum as the author. In the engine exhibited the weights of all the parts were reduced to a minimum. Even when so reduced, he took care to keep up the rubbing or wearing surfaces to a maximum. As to the question of how far the principle could be carried out in a large ship, it would be impossible to employ the system for the Channel service. There it was absolutely necessary to have a machine that would last, and that would give no trouble. As to jacketing, he thought it might be used with a saving of 30 per cent. of fuel, and a saving of 30 per cent. in the boilers, and that the saving of weight of the boilers would be such as to compensate for the extra weight of the jacketed engines.

Mr. William John wished to express his admiration of the work which Mr. Thornycroft and Mr. Yarrow had done. They had set all naval architects thinking, and the question had naturally passed through their minds, if it was possible to get a speed of 20 knots in a little boat, and the same speed in a big boat, why not in a ship of intermediate size? Mr. White had pointed out the

difficulties in the way; Mr. John would not say they were insuperable, but the desired object could not be obtained with moderate-sized ships at the present time. With regard to the subject of vibration, he remembered some years ago being in one of Mr. Thornycroft's torpedo boats, and noticing that the effect of vibration at half speed was much greater than at full speed, and that there were very great differences in the vibration according to the variations in speed. That had been attributed in part to the revolutions of the screw synchronizing at some speeds with the natural vibration of the boat. He should like to know what the natural vibration of a boat was. He had taken observations on board ships several times as to the vibration, and it was a difficulty he had always experienced. In a tuning fork, or in a substance fixed at one end, there was a natural vibration; but in the case of a vessel floating free at both ends there was an entirely different time of vibration at the bow from that at the stern. He had observed the stern vibrating twice as rapidly as the bow. As to the question of stability, he had recently had to investigate the stability of some torpedo boats built by Mr. Yarrow, and he could bear out the statement of Mr. Thornycroft that the boats had a very large range of stability, and a great safety against capsizing. There was one other point to which he wished to refer. He did not think the facts would bear the generalization that had been put upon them in the paper. "To augment the stability, the width of the deck may be increased considerably beyond the width measured at the water line. The increase of bulk thus effected augments the righting force, when any considerable inclination of the vessel from the vertical is reached, without impairing the speed." That was often the case, but not necessarily at all large angles. It was so generally, however, with torpedo boats, yachts, and other small craft. But the width of the vessel above the water line might be increased and the range of the stability diminished. He thought that point was not sufficiently appreciated. By increasing the breadth of the deck the weights above water were naturally increased, the center of gravity was lifted, and there was a reduction of initial stability and range of stability in that way. But beyond that, even without any change in the position of the center of gravity, the effect might be produced of reducing the range. For instance, if a cylinder was loaded at all eccentrically, if the weight were put so that the center of gravity would come below the center, the cylinder had practically all the properties of a life boat; there was only one position of stable equilibrium and one position of unstable equilibrum, so that the curve of stability would practically have a range of 180°. If the center of gravity were lowered the range would be kept the same, and the curve would be increased. What occurred when the breadth above the water line was increased in such a case might be as follows: Supposing a broad deck to be put on to the vessel without adding to the weight, what was obtained was a vessel which, when turned bottom up, would practically have a broader water line than before, and would really have stability in the bottom up position, so that here was a body that was in stable equilibrium when it was floating the right way up, and also in stable equilibrium when it was floating bottom up, and there must be an intermediate position of instability. The range must therefore have been reduced from 180° by the change made. He only gave an indication of what might occur, but he thought it was a point worth considering when drawing general inferences as to alterations of stability from alterations of the form above the water line.

Mr. F. J. Bramwell, Vice-President, said the author had done him the honor to refer to certain experiments he had made nine years ago with one of his early boats, the "Miranda." That boat gave results which up to that time were entirely unknown, results which it had been stated by high authorities on naval architecture could not possibly be attained. In fact it was well known that one gentleman had said a boat under a given length could not be propelled beyond a certain rate by engine power at all, without a cost so excessive that it would be cheaper to carry it on men's shoulders. The author, by the construction of the "Miranda," showed that those statements were inaccurate, and that it was possible, by a judicious con-

struction of the hull and a due proportionment of engine and boiler power, and by having regard to the weight of the structure, to attain the speeds which had previously been thought to be, with vessels of the size with which the author was then dealing, beyond the range of possibility. He exhibited a copy of one of the diagrams referred to in the Transactions of the Institution of Naval Architects, before which institution his paper in reference to the experiments on the "Miranda" had been read. Other diagrams accompanied that paper, but he thought the one now exhibited would suffice for the present meeting, as it contained the gist of the experiments. It would be observed that there was shown by the sloping line the gradual increasing speed of the screw, which was of 3 feet 4 inches pitch, and thus, at the six hundred revolutions, attained a rectilinear velocity of 2,000 feet in a minute. Experiments were made at two hundred, three hundred, four hundred, five hundred, five hundred and fifty-five, and six hundred revolutions, and the speeds attained were respectively 7 miles (not knots), nearly 10, nearly 12, 16¾, and 18⅜ at five hundred and fifty-five revolutions. The HP. developed was also shown to be from 4 HP. at two hundred revolutions up to 71 HP. at six hundred revolutions. It would be seen that the growth of the HP. at the increasing revolutions was such as to present a fair curve. With respect to the slip of the screw, this was only 7 per cent. at two hundred revolutions, but increased to 13 at three hundred, and got up to 21 at four hundred, then decreased to 14.7 at five hundred, and to 11.3 at five hundred and fifty-five. The author had mentioned that, having regard to the apparently anomalous result obtained by the falling off in the slip, and the apparent increment of speed of vessel compared with the speed of the screw as the pace got greater, the experiment was repeated, and the repetition showed the accuracy of the results previously recorded. The experiments were made with very great care, and he thought that they were in every way trustworthy. It was at first found somewhat difficult to obtain the indicator diagrams, but by a little management they were got with a great degree of certainty at six hundred revolutions per minute. The management consisted in using a very strong spring, so as to limit the vertical stroke of the indicator to get rid of the momentum of the piston and other parts of the indicator, and also in using a supplement to the rotating spring of the indicator cylinder, and in being content with a very short diagram, the intention being that the diagram should not exceed one inch in height and one in length. At six hundred revolutions it was not possible to see the indicator at work; it disappeared in a sort of "burr"; but the diagram was quite clear, and could be readily worked from, giving accurate indications of the power, as was proved by the repetition some weeks after the first diagrams were taken. Since that time, the author, having special regard to torpedo purposes, had improved his boats by making them condensing. The "Miranda" was not a condensing boat, and was, therefore, liable to the defect of exhibiting a stream of escaping steam—exhaust steam— which would render it visible to an enemy. The problem of making engines of the high speed of six hundred revolutions a minute, or even of the lower speeds obtaining in larger powered engines, condensing, was a somewhat difficult one if the air pumps were to be worked off the engines, which the author wisely preferred; but he had shown that by a judicious proportion of the valves and the stroke of the air pump to that of the engine, it was feasible to derive the motion of the air pump from the engine, and to procure all the benefits of condensing for the engines. They had since been made compound, and in that way the quantity of steam needed being diminished, the size of the boiler had not been increased in the same ratio as the power of the engines. He gathered that the author had, by bringing his paper before the Institution, proposed to do two things—one to tell the members frankly what he had done, the other to ask for comments and criticisms as to what might be further done in the way of improvement. He believed the author was not wedded to any particular system, and had no objection to take advice, but was glad to hear all that could be suggested. The author had called attention to the benefits that would attend reduction in weight. With respect to this question,

Mr. Bramwell did not find in the paper the actual statement of the weight of the boiler and of the water in the boiler, but no doubt it must be a very large percentage of the total weight of the engines. He had, however, endeavored from the information afforded in the paper, to calculate the weight of the boiler and of the water in the first-class torpedo boat. Obviously where there was a boiler of the locomotive construction, the exterior of the boiler must be made of plates of sufficient strength to bear the pressure. A boiler four feet five inches in diameter, working at the pressure at which the author's engines worked, could not be dealt with lightly, and therefore a considerable weight was of necessity involved in the construction of that kind of boiler. Moreover, the whole of the plate surface which confined the water and the steam was inoperative as heating surface, and therefore the very surface which demanded the greatest amount of metal and added most to the weight of the boat, was surface which in no way tended to generate steam. The nature of the boiler also demanded that a considerable quantity of water should be accumulated in it, in order that the tubes and the top of the fire box might be covered. He was well aware of the value of a considerable quantity of water in a boiler, as being in itself the best reservoir of heat for the boiler, and he was also aware that if the amount of water in the boiler could be diminished it would be necessary proportionately to increase the watchfulness of those in charge of the engines, unless they resorted to boilers like the Herreshoff boiler, where there was no store of water, but a mere injection, nearly all of which was turned into steam. This was going back to the early type of injection boiler. He referred to the so-called Quicksilver engine, in which the boiler was provided with a double bottom, the space between the two plates at the bottom being filled with an amalgam of quicksilver which absorbed the heat from the fire at altimes, and gave it out again at each half reciprocation of the engine to the water which was projected upon the upper heated surface—an indented surface—and was at once turned into steam, and was immediately introduced into the cyl inder. This construction of injection-boiler worked very well, he believed, for a year or two in a boat trading from London to Ramsgate. The engines were made by Penn; the invention of the amalgam to absorb the heat of the fire and to store it up until delivered to the wtaer as injected, was the invention of the late Mr. Howard, of the King and Queen Ironworks, Rotherhithe. But even if a boiler of the class he had mentioned—one of those which did not contain water, but which simply served as heaters of water—were not employed, the question then arose, if lightness were sought (and it was clear from the statements of the author and other speakers that in such boats lightness was of the essence of speed), should the use of the locomotive boiler be continued, or should some water-containing boiler, where all the surface was steam-generating, be resorted to? On this point he desired once more to refer to another boiler of which he had so often spoken, namely, Hancock's. As far as he could make out, a boiler of that construction would be about 2½ tons lighter, taking into account the boiler and the water, than the boiler (and water) would be if of the locomotive type employed in the first-class boat as described by the author. He would not weary the members by repeating the construction of the Hancock boiler, but he might mention that the whole of the surface was used in generating steam. It was true it involved a non-conducting envelope round the fire. One like that used by Mr. Perkins for his boilers would be admirably suited for the purpose, consisting of two thicknesses of iron with vegetable black between. An envelope of that character would admit of a high heat on one side, while the hand might be harmlessly put upon it on the other side. If a Hancock boiler thus cased were used he believed that 2 to 2½ tons would be saved in the weight of the boiler and the water in a first-class torpedo boat. The question also arose whether a further saving could not be made by venturing upon higher pressures than were used by the author at present. It appeared to him that if those higher pressures were employed, more effect would be obtained out of the steam. He did not wish to refer to the economy of fuel when the object was to launch a torpedo against an enemy, as in such cases it mattered ve_y little whether

1s. or £1 were paid for the fuel. In fact it was unnecessary to consider economy of fuel on such occasions. Yet, if it was practicable to have a more economical system of engine by the use of higher pressures, it would end in a smaller and therefore a lighter boiler. It would also lead to this, that the fuel carried would afford a greater number of hours' steaming than would be possible with a less economical engine. Upon those grounds, therefore, and not upon the ground of the desirability of saving a few shillings or a few pounds in the use of the boats for the purposes for which they were required, he thought it would be well if the author would devote his attention to the construction of a different form of boiler from the locomotive boiler, and to the adoption of a higher pressure of steam. In those two ways, he thought a reduction of weight, which appeared to be the great desideratum, might be looked for. He did not see how the engines themselves were to be reduced in weight except by an increase of piston-speed, which at present was very high; and although he was far from saying it might not be made higher, he should be inclined to seek economy in weight in the direction he had indicated, rather than seek it, in the outset at all events, in an increase of speed of the engine.

Mr. W. Schonheyder thought the statement in the paper that "a relatively high speed of engine may be taken as favorable for economy of steam, in consequence of the smaller area of cooling surface in the cylinders of high-speed engines than in those of low-speed engines exerting equal power," required some further explanation. In all steam engines there were two losses: the external loss due to radiation, and the internal loss due to the condensation from work done during expansion; and in order to decide if it was of advantage to work an engine at a high speed or a low speed, it was necessary to distinguish between engines that were jacketed and those that were not jacketed. In an unjacketed engine it was an advantage to work fast, inasmuch as the extent of cooling surface became small in proportion to the amount of steam passing through the engine in a given time; therefore the condensation was less, and the engine worked more economically. But in an engine that was steam-jacketed it might be, and often was, a great advantage to drive the engine slowly, because then the amount of jacket surface which was available in proportion to the amount of steam passing through the engine in a given time became positively larger, and prevented the large amount of condensation which took place in engines driven at a high rate of speed. It was for that reason that many engines, at a comparatively low pressure, 40 lbs. or 50 lbs. per square inch, had done exceedingly good duty at low speeds. It appeared to him that the question of jacketing was very little understood, but he thought that the whole thing was in a nutshell. An engine which was surrounded simply by an envelope of steam was called a jacketed engine, but very little account was taken of whether the engine exerted a large power or a small power. The jacket was wanted for the purpose of supplying the heat which was lost in the cylinder when the expansion took place, and whether the engine was working at ten or at one hundred revolutions a minute, the jacket ought to be made in proportion to the amount of heat which was to be imparted to the working steam. He believed it was the first time that attention had been directed to that particular point. As jackets were, as a rule, simply envelopes round the cylinder it was no wonder that people sometimes found that they were of very little use, and in the engine which the author had described he considered the jacketing, as usually carried out, to be quite useless. The author had found no advantage in the jackets, simply because they were so arranged that it was impossible to impart sufficient heat to the steam in the cylinder to make up the loss by condensation; but there was a positive advantage in jacketing when well carried out. He wished to quote some experiments by Messrs. Bryan Donkin & Co. upon an engine tested with jackets and without jackets at the same pressure. With jackets there was a development of 46.21 indicated HP.; without, 27.78 HP., the same pressure being used in both cases, and the vacuum being practically the same. The water evaporated per indicated HP. per hour was 22.51 lbs. with jackets, and 32.72 lbs. without jackets. The author had directed attention to ex-

periments made in America with the steamer "Rush," in which the cylinders were steam jacketed, with 82 lbs. per square inch initial pressure. The most economical ratio of total expansion was 6.22. If the jacketing was insufficient, only a small amount of expansion could be carried out. With regard to the screw propellers, he did not say that the propeller shown by the author might not be a very efficient one for torpedo boats; but it imparted a large amount of rotary motion to the water. Attention had been called to that point by Mr. Yarrow, who referred to the stationary guide blades as having been employed for stopping that rotary motion. These guides were invented by Mr. Arthur Rigg, in 1863–4, who found a great advantage in using them, as they stopped the rotary motion of the water, and converted it into an after motion. But when they were employed it was necessary to make the screw and the guide plates of a proper shape.

Mr. George R. Dunell said any one who had seen the quickness with which Mr. Herreshoff's boat moved when the engines were started, the readiness with which the vessel stopped, and the command which the propeller seemed to have over the boat, would acknowledge that it must be placed in a very advantageous position. It was principally with a view to the manœuvering properties of the boat that the screw was put under the boat. It would be remembered that the vessel was steered in circles of very small radius, going either ahead or astern. The fore and aft trim, when going at a high speed, was also kept much better than was usual in torpedo boats, and that was largely due to the position of the propeller. Valuable as Mr. Yarrow's experiments were, he thought they could hardly be said to bear upon Mr. Herreshoff's boat. Mr. Yarrow had used a straight shaft, while the shaft of Mr. Herreshoff's boat was bent, or rather, sprung; so that the screw was kept nearly at right angles to the water line of the boat. That sprung shaft was the most surprising feature in the boat; but that it was a desirable arrangement required, in the opinion of many, further proof. It was the result of six or eight months' continuous experiments on the part of Mr. Herreshoff. Mr. Dunell was present at Portsmouth when the shaft was taken out of the torpedo boat for examination. After running for some time, it was confidently expected that it would be worn down to a considerable extent, but the tool marks were visible in the shaft after that long running. The boat obtained, on the official trial trip in the Thames, a speed of nearly $\frac{3}{4}$ knot higher than had been reached by any boat of the class on an Admiralty trial trip up to that time. He wished to be allowed to refer to a boat built by Mr. Herreshoff for the American government in 1876, and described in the Annual Official Report of the Secretary of the United States Navy. It was built of wood; length over all, 58 feet; beam, 6 feet 3 inches; depth, 3 feet 2 inches. There was one Herreshoff boiler, and a pair of ordinary non-condensing launch engines; the cylinders were 5 inches in diameter, with a stroke of 10 inches. On a trial trip, the boat ran 20.24 statute miles in 59 minutes and 43 seconds. On the down trip the steam pressure was reported steady at 140 lbs. per square inch; and on the return trip, the average pressure was only 100 lbs. A small pipe connecting the steam guage gave way, and it was plugged with a piece of pine without the boat being stopped, low-pressure steam being generated meanwhile. The "Miranda," the parent of all fast launches, was 50 feet long, and the beam on the water line was 5 feet $9\frac{1}{2}$ inches. The vessel had a pair of engines 6 inches in diameter by 8 inches stroke. It was, however, to the comparative weight of the machinery in the two boats that he wished to call attention. According to a paper read by Mr. Bramwell before the Institution of Naval Architects in 1872, the weight of the "Miranda's" machinery, with the water in the boiler, was 4,500 lbs. The total weight of the machinery in the American boat, exclusive of the water in the boiler, was 3,132 lbs., and, allowing 268 lbs. for the water in the boiler, probably double the actual weight, the difference in the two weights was 1,000 lbs. in favor of the American boat. From what he had seen of the "Miranda's" engines, he should judge that they were at least as light as those in the American boat; and he thought there was little doubt that the difference in weight was due

to the lightness of the Herreshoff boiler and the small quantity of water which it contained. The American boat was eight feet longer than the "Miranda," and 4½ inches wider; the speed was, as nearly as possible, 2 miles an hour faster than the "Miranda's." The trial trip took place in salt water, with non-condensing engines; the distance was 20 miles, and the boiler was fed with salt water the whole time. He did not wish to make a comparison of the speeds, but rather to draw attention to the difference in the weight of the engines. There was no record that diagrams were taken of American boats' engines, and he could not therefore bring the comparison to a very fine point; but he thought he had said enough to make out a *prima facie* case to show that something might be gained in speed by the use of the Herreshoff boiler. The American boat had been running since 1876, and, he believed, was running still, having used salt water the whole time. There was a method of detaching the scale by heating the coil. The coil would bear heating without damage, and, by the metal expanding, the scale was cracked and blown off. Still, as a rule, the use of salt water was not recommended, for it was difficult to get the people in charge to pay proper attention to keeping the coil free from scale. He was glad to say that Mr. Thornycroft was about to put Herreshoff boilers into two of his boats at the request of the Admiralty, so there would then be an opportunity of seeing what a boiler of that description would do in conjunction with the best machinery for that class of work. As to the consumption of coal, Mr. Donaldson had said, in a paper read before the United Service Institution, that he thought the Herreshoff boiler would be more wasteful as regards fuel than the ordinary boiler. Mr. Isherwood, in America, had completed some extensive experiments on coal consumption in the Herreshoff boiler, in which he had obtained 1 indicated H.P. with 2 lbs. of coal per hour in a small yacht 100 feet long, with compound condensing engines having 9-inch and 16-inch cylinders with an 18 inch stroke. A report would shortly be published by the United States naval authorities on a series of experiments made on some vessels fitted with Herreshoff boilers, and extending over a period of three months, the boats running from 5 or 6 o'clock in the morning until 9 o'clock at night, twenty-four thousand indicator diagrams having been taken. It had been stated that the Herreshoff boiler was simply an old invention resuscitated after having been cast aside as worthless. There had been pipe boilers, coil boilers, and other water-tube boilers of various kinds; and, no doubt, to casual observers, they appeared much alike. The gentleman who remarked on the antiquity of the Herreshoff boiler, as an amateur experimentalist, was not perhaps required to be so searching in his inquiries as those who had more material interests at stake. There had been a great many inventions in high-pressure steam made by the very talented family to whom reference had been made; but the invention of the Herreshoff boiler was entirely due to Mr. Herreshoff.

Mr. J. F. FLANNERY felt it ought not to be forgotten that Mr. Yarrow was the first to attain a speed with his vessels of 22 knots an hour, although Mr. Thornycroft had been the first to attain a speed of 19 knots. At present it was difficult to distinguish between the labors and the results obtained by those two gentlemen; and in considering and placing on record the strides that had been made in modern machinery, introducing almost a new era, he thought that the names of Mr. Thornycroft and of Mr. Yarrow should be mentioned side by side, and he believed that no one would more cheerfully agree to that being done than the author himself. Mr. White had shown how the reduction of weight in machinery reacted upon the size of the vessel necessary to perform a given duty: how by reducing the weight of the machinery the necessary size of the boat was reduced, and how that again in a secondary degree reduced the size and weight of the machinery. He did not think it was possible from a naval architect's, or from a shipowner's, or from a marine engineer's point of view, to exaggerate the importance of lightness. It appeared from the paper that lightness had been obtained in torpedo boat machinery by two general means. The reduction in weight of the machinery was effected by increasing the piston speed, and the reduction in weight of the boiler

by increasing the rate of evaporation per square foot of fire grate. The author did not indicate that in his opinion there was any loss of efficiency by increasing the piston speed, and Mr. Flannery would point out that the high speed which had been obtained, 880 feet per minute, might be possible with other than small engines. In the "Parisian," which was recently tried on the Clyde, the stroke of the engines was 5 feet, and the number of revolutions was allowed by Mr. Kirk, M. Inst. C.E., the designer and builder, to increase to as many as eighty-five per minute, thus giving the enormous and unprecedented speed of 850 feet per minute for an engine that indicated 6000 HP. It had thus been shown that under certain conditions it was possible to increase the speed of the piston enormously in large engines in comparison with those running at the present time. In regard to the increase in the rate of combustion, and to the reduction in the weight of the machinery by that means, where the author had a pressure of a blast corresponding to a height of 2 inches of water, the coal consumed per square foot of fire grate amounted to 49 lbs.; and where he had a pressure corresponding to 6 inches of water it amounted to 96 lbs. This tended to show that although the blast was increased considerably, a proportionate increase in the rate of combustion could not be expected. At the same time the economy was reduced by one-seventh. That disadvantage had been clearly pointed out by the author. Another disadvantage might also be inferred from one of the lines in the table. It would be observed that when the low pressure of the blast was applied to the boiler, the duration of the experiment was two hours; but that when the higher pressure of blast was applied, the duration of the experiment was one hour and twenty-seven minutes. He was not unprepared to hear that the reason why the latter experiment had been so much shorter than the former, was that the bars were considerably injured by the intense local heat generated by the blast. He was prepared to find that with those enormous blasts the bars would be injured, and if that were so it would be a reason for not anticipating that the weight of the boilers would be reduced by largely increasing, by those means, the rate of evaporation and the rate of combustion per square foot of firegrate, unless some means were adopted to preserve the bars while the fire was subjected to the action of the blast. Mr. Wright had given some weights relating to the "Polyphemus," and it was generally known that other vessels of a similar character were being built by the Austrian Government. Mr. Flannery should like to add that there were four cylinders in those engines, 28 inches in diameter, of 33 inches length of stroke, the number of revolutions being one hundred and forty, and the piston speed 770 feet per minute. There were five locomotive boilers made of steel, with copper fire boxes working at a pressure of about 100 lbs. per square inch. The indicated HP. was 2000. The weight was 225 tons, corresponding to 280 lbs. per indicated HP. Those figures, and the fact that the Austrian Government was moving in that direction, tended to show that other nations besides England were alive to the importance of deducing proper lessons from the results that had been obtained; and, if that was taken to heart by those who were dealing with a larger class of work than the author had dealt with, his paper would not have been written and read in vain. He hoped that he should not be misunderstood in what he had previously said, because he desired to bear testimony in the very strongest manner to the liberality with which the author had laid before the Institution the details of his work which hitherto had been kept secret.

Mr. Thornycroft, in reply, said he wished first to refer to the diagram of some experiments made on the "Miranda," and which were alluded to by Mr. White and Mr. Bramwell. The lower increase of power required for high speed was shown by the reduction of slip in the propeller, and the different value of the ordinates. Mr. Bramwell had remarked on the indicator diagrams taken at high speed, and he could only corroborate what Mr. Bramwell had said as to the reliability of diagrams of that high speed if they were made sufficiently small. Mr. Bramwell, had also called attention to the fact that in the locomotive boiler of first-class boats, a great part of the weight consisted of surface that was

not productive. Mr. Thornycroft regretted that the surface could not be made productive. Mr. Bramwell had mentioned a kind of boiler which he said would probably save 2 tons in weight. The boiler, he believed, weighed a little over 5 tons, and the water nearly 2 tons. Of course, if they could utilize the whole of the surface for generating steam, and only have a light case to contain the products of combustion, the boiler would be lighter; consideration would be given to the suggestion, with a view of lessening the weight. He believed the total weight of the boiler and water was about one-half of the total weight of the machinery. Mr. Bramwell thought that by using high pressure the engines might be reduced in weight. Possibly that was so, but Mr. Crampton years ago recommended them not to depart from the present practice, because of the great trouble experienced in packing. He was not sure whether asbestos packing would get over the difficulty, but they had not ventured beyond 130 lbs.; 140 lbs. was used in locomotive practice, but the engines were not used in full power for any considerable time. Mr. Schonheyder had referred to jackets, and his opinion was that if the engines ran slowly the jackets would have time to work. He need hardly say that with torpedo boats they could not afford to run the engines slowly in order that the jacket might have time to do its work. Whether or not there was any advantage in the rings to which Mr. Schonheyder alluded he did not know. Mr. Dunell had referred to the Herreshoff boiler, and had mentioned that his firm was going to fit that boiler to two boats. That was the case, but he would not venture to prophesy what would be the result. As to the effect of the high center of gravity, which Mr. Bernays seemed to think an important feature, the weight of the cylinders in comparison with the whole weight was really so small that the height of the cylinders did not affect the stability much. Mr. Yarrow had asked what was the consumption of coal for long distances. His firm had not accurate information as to the consumption of coal for a long run, but in the first-class torpedo boats between London and Cherbourg they had found that at 11 knots the consumption was about $2\frac{1}{2}$ tons. Mr. Yarrow had also spoken of the screw in the position used by Mr. Herreshoff, under the center of the boat; he thought that considerable loss was occasioned thereby. It did not appear to him that Mr. Yarrow had thoroughly appreciated the object with which Mr. Herreshoff had bent his shaft, which was to bring the screw about perpendicular to its work. The diagonal position of the screw had more effect than would appear at first sight. Taking an extreme case by way of illustration, if the axis of the screw were inclined to the line of motion 45°, that part of the screw blade which had a pitch of three diameters varied in pitch throughout the revolution, and the amount that it varied in the present case was from nothing to infinity. Mr. Barnaby had called attention to the stream line motion of the water about the boat, and stated that he did not know whether Mr. Yarrow had adapted the screw properly, and that if that was not done there would be a further loss. He had also referred to the subject of vibration, and said that a torpedo boat of 86 feet in length vibrated when the engine ran at two hundred, four hundred, six hundred, or eight hundred revolutions. Their experience with boats of this kind had been that there was no considerable vibration when the engines were running so slowly. In a boat of that size they found that the vibration was considerable when the engines attained a speed of three hundred and sixty or three hundred and seventy to three hundred and ninety revolutions, and that it began to diminish at four hundred. The strength of the boat had no doubt some influence on the result.

Mr. Yarrow had said that the only point in which he differed from the author was as to the advantage in placing the propeller before the rudder, and he had instanced the results of experiments in support of his argument in favor of this disposition. Mr. Yarrow appeared to think that there was some advantage in simply stopping the rotation of the water generated by the propeller; in fact he attributed to the rudder an action similar to that of a properly constructed guide plate. A flat rudder no doubt stopped the rotation of the water, but it was not possible to get any propelling action from a surface, which was

perpendicular to the motion of the vessel. Messrs. Rankine and Napier had patented a special form of rudder in which the blade was curved so as to take advantage of that transverse motion of water, but whether any distinct advantages had been obtained from it he had not heard. In the action of an ordinary propeller, where the blades were narrow in proportion to their length, measured at right angles to the axis, the water passing through the disc had rotary motion imparted to it, so that during part of the revolution the guide plates could be efficient, but during the other part they might be a positive resistance. It appeared to him that experiments on two boats of similar construction might give such results as could not be regarded as conclusive. In some models he had made, the guide plates and cylinders slid loosely on the shaft. Being aft of the propeller and prevented from revolving by a tangential wire, when they pulled forward they pressed against the propeller, and by a self-recording apparatus the whole thrust was taken—the thrust due to the guide plates less friction; because before the guide plates could exert any thrust upon the shaft they had to propel the tube and overcome their own resistance in the water. It was found that the effect which still remained due to the guide plates was about one-fifth of the whole propelling effect; the propeller was intended to do what the late Mr. Froude proposed to do with the paddle wheel. Mr. Froude had explained to him a form of paddle wheel he had proposed, and which impressed gradually on the water the motion to be given in propelling. If the water was started in an abrupt manner by a blow, the loss by slipping amounted to the whole speed of the slip; in other words the work was all done at the speed at which the water was finally discharged. But if the water could be impressed with motion gradually, the loss by slip would only amount to one-half the acceleration, and would be reduced to one-half that of the previous case if the final velocity impressed were the same in both cases. Mr. Froude proposed, in immersing the blade of a paddle wheel in the water, to give each blade a gradually increasing motion by proper mechanism, and he thought "this would not be difficult to devise" for the screw propeller. But he found that Mr. Bramwell's specification described the method of constructing paddle wheels in which gradual acceleration of the water was the chief aim, and Mr. Bramwell considered that better results might be obtained, and that possibly the same thing could be done with a screw. Mr. Froude did not appear to have arrived at any solution of the question for the screw propeller, but Mr. Thornycroft afterwards thought it could be done with a screw if a suitable channel could be made for the water to flow through. The water went into the propelling apparatus at the speed found at that part of the vessel, and the area should be so proportioned that the water would be discharged at the required velocity to propel the ship, the propeller gradually increasing the velocity. With the ordinary propeller, acceleration took place to a great extent before the water arrived at the propeller, and the propeller had to act in water running at the accelerated speed. The curious part of the result was that, with the propeller represented, when using twice the acceleration of the common propeller, and having only half the area of disc, about the same efficiency was obtained as that from an ordinary screw when working at its greatest efficiency.

With regard to the tubes of the boilers, they had, as Mr. Wright had explained, some unsatisfactory work. The tubes had leaked into the fire box, but that leakage, together with some priming, was almost the only trouble they had experienced. Iron tubes appeared to form a certain quantity of oxide in the water, producing a liability to prime. They believed that copper fire boxes gave more steam, but they did not consider that they were so strong as iron fire boxes. Mr. White thought that there were two obvious limitations to the extension of the torpedo system—the hull being very light and the load being very light. He considered that small hulls must be heavier than large ones, and he would prefer that the author should compare torpedo boats with tugs and small launches instead of with large vessels. Mr. Thornycroft thought that tugs and small launches were not suitable for comparison; the former were able to stand rough usage, and were intended to bump

against larger structures; but torpedo boats were not designed for any such purpose. The hulls of the torpedo boats had been found amply strong, and had not given way under ordinary treatment. They had been perforated once or twice. On one occasion, in the dark, one torpedo boat ran into another at a considerable speed, and it would not be considered a surprising matter that one of them went right through the other; the bows of one boat were smashed, and the sides of the other were perforated. The water-tight compartments were intact, and neither of the boats sank; both floated without repair for about a day. At Cherbourg, where they had delivered some boats for the French Government, one of them ran into the dock wall at a speed of 12 knots an hour; the bows were smashed and the stokers and engineers were hurt by the sudden stop, but the boat still remained watertight and seaworthy. One of the English boats ran ashore at Malta, and her bows were smashed, but the machinery was not damaged, and the structure was not injured, except that part of the bow which was smashed. Mr. White appeared to think that small boats ought to be the heaviest. The strength of a boat only increased as the cube of her length. The vessels being similar, the stresses among the waves of the sea varied as the fourth power. To confirm the correctness of that statement he would refer Mr. White to a Paper by Mr. John on the strength of iron ships, read before the Institution of Naval Architects in 1874. "The tension per square inch on the upper works of a vessel of 3,000 tons appears to be once and a half as great as that on a 1,500-ton ship, twice as great as on a 700-ton ship, and five times as great as in a little vessel of 100 tons." The torpedo boats were not even 100 tons. Mr. White had asked if the hulls of the second-class boats were lighter than those of the first-class. They were lighter. In the paper they were stated at 30 per cent., the first-class being 33. As to durability, the author had built a boat containing some plates weighing only 1 lb. per square foot, or $\frac{1}{40}$ inch thick, and they had lasted twelve years. No doubt if they had been allowed to rust they would not have lasted so long; but such plates should not be allowed to rust. Mr. White considered that the hull was a mere shell carrying the machinery, but this was not so; it was divided by a number of efficient bulkheads and half bulkheads, which greatly increased the strength of the hull and lessened the danger of sinking. With respect to the load, high speed passenger vessels carried, he thought, even less load than torpedo boats. Mr. White appeared to think that the loss of speed due to the increased load was peculiar to torpedo boats. Of course, the diminution of load increased the speed of all vessels. In regard to the question of economy, he did not think that Mr. White had made a sufficient distinction between the engine and the boiler. The engines of the torpedo boats were compound, and they were economical in the quantity of steam used. The ratio of expansion perhaps was not so great as might be used where the cylinder capacity was not limited; but a further increase of cylinder capacity to allow a larger ratio of expansion, where only two cylinders were used, he did not think was a very great advantage. Mr. Crampton had said that the boiler could not be made economical; but where a particular service had to be performed, it was easy so to proportion the boiler and the fuel as to get the least combined weight of the boiler and the fuel. In those boats, of course, the fuel had only to be carried for a short distance. Mr. White thought that Mr. Thornycroft was surprised at the results obtained in the "Miranda." He had built the "Nautilus" in 1860, and at that time there were boats running on the Regent's Canal which had been imported from Lancashire, and these boats ran nearly on the wave. In building the "Nautilus," he had hoped to get the same result in a steamer, which was got by the canal boats. These were 6 feet wide and 60 feet long, and were propelled by two horses at the rate of about 9 or 10 miles an hour. The effect was that the water in the canal was very little disturbed at that high speed. The speed of the principal waves which accompanied the boat was limited by the small section of the canal. Where the same thing was attempted in open water, the circumstances were different. He had hoped in the "Nautilus" to accomplish that result. She ran in the University Boat

Race of 1862, at a speed of 11 or 12 miles an hour. In 1870 he had built a fast boat, the "Ariel," which had a light engine and a framing of wrought iron, and some plates in the hull $\frac{1}{4\,0}$ inch thick. The boiler was subject to priming, and the speed of the boat was limited by the steam-generating power of the boiler. The speed actually obtained was 14 miles an hour, but the boat did not mount the wave. Mr. White wished to know how Mr. Thornycroft would apply in a moder ate draught of water the large powers which he had succeeded in calculating to drive the "Iris" at 40 knots. Mr. White had estimated that it would take 45,000 HP. No doubt, he did not at the time think of the great advantage he was giving to the propeller. The members might be surprised at the smallness of the diameter of propeller which would give the best propelling effect. A propeller like that on the table had been fitted to Her Majesty's torpedo boat "Lightning;" the diameter of the tube was 3 feet, and at 18 knots 400 H.P. was utilized to the best advantage. Two propellers, similar to the model for the "Iris," each 7 feet in diam-ter, would be required to use 45,000 HP. at 40 knots. Encouraged by Mr. White's remarks, Mr. Thornycroft had been induced to calculate what could be done in a moderate draught, and he had found that in the case of a large vessel going 50 knots, if two 20 feet diameeter guide plate propellers were used, they would be working at the best efficiency in utilizing 762,000 HP. Mr. Samuda had stated that he had been induced to use simple engines instead of compound, because of the increased weight of the compound. No doubt he had good authority for that statement, but he thought that in that case the limited number of revolutions allowed by the paddle wheels were the cause of the increased weight, the addition to the cylinders being greater than the reduction of the boilers by the change. It appeared however to Mr. Thornycroft that if paddles could be dispensed with and a propeller used which would allow the engines to be run at high speed, compound engines would undoubtedly have the advantage in reduced weight. Two guide-blade propellers on his design would only require to be each 5 feet 6 inches diameter for the power mentioned by Mr. Samuda, and a speed of 18 knots.

Mr. John wished to know what was the natural vibration of boats. The worst he was acquainted with was one instance in which there were two transverse sections of the boat that were nearly still, situated about one-third, or rather less, from each end; while each end and the center section had considerable motion, all of the same period, so as to cause a line intended to be straight in the length of the boat to be alternately convex and concave in a vertical plane. He was sure that boats did sometimes behave in that way, having distinctly felt the still places by walking along the deck. He had been interested to hear from Mr. John that sometimes there were double the number of oscillations at one end than at the other. Mr. John had taken exception to the language which he had used, and which was, perhaps, a little too general. Mr. John had shown a particular case in which the range of stability was lessened, but even in that example the greatest righting moment was increased. In the torpedo boat referred to, as the stability vanished before 90°, and as the added bulk was above the center of gravity when the boat was inclined to 90°, stability was added. He felt sure that at lower angles it was added, and therefore the range of stability was increased by that addition, so that the reasoning Mr. John had given for a cylinder loaded below its center and increased in bulk above did not apply to vessels having ranges of stability less than 90° from the vertical.

The question had been asked why jackets were not put on the second-class engines, and were put on those of the first class. It was desired in that instance to make larger engines without altering the patterns more than necessary, and the liners were taken out. The main bolts, the connecting rods, and the parts that required strengthening, were made larger, and the engines, although occupying the same bulk, had high-pressure cylinders 8 inches instead of 7 inches in diameter, and the low-pressure cylinders were also increased. The engines never had steam jacket covers, but if the engines were jacketed the covers ought to be jacketed also. That had been avoided on account of the weight;

but in order to make the covers strong they found that they necessarily became double, or nearly so. In more recent engines the covers and the bodies of the cylinders were jacketed. They had used a solid, square cast iron piston valve, working through a hole which fitted it accurately. He had thought that one of those valves would one day stick fast and cause the engine to break down, but that had not been the case. They had found that Perkins' metal rings wore away, and on the slide valve there was a fine bronze powdered dust, which seemed to have the effect of aiding the lubrication. They did not intentionally put any oil in the cylinders. Some oil got in at the piston rod and other glands, and then reached the boilers. They were going to try the effect of not putting oil on the piston rods. Captain Dawson had asked how many hours they could run at 20 knots an hour. He thought about four hours. With the boats tried at Cherbourg, they had guaranteed three hours at full speed—18 knots, and coal was carried for that purpose. Of course they could have carried coal for a greater distance. Mr. Spencer had inquired what margin of safety was found in the engines described in the paper. In the line shafting it was estimated that the streets at the surface did not exceed 12,000 lbs. per square inch; in the piston rods when under compression some part of the section might have a load equal to 12,400 lbs. per square inch, but when under tensile load it might be supposed that the stress was equally divided over the section, and it would not then exceed 6,500 lbs. per square inch in the cylindrical part of the rod. In the connecting-rod and in the main bolts a factor of safety of about 6 was used, and the practice was justified by the absence of accidents. With regard to the factor to be used in long shafts where a heavy propeller turned an equivalent to a fly wheel at one end, and the engines had a large moment of inertia at the other, the elasticity of the shaft, together with the unequal resistance of the screw during different parts of its revolution, and the irregular turning power of the engines during their revolution, were competent to cause much more stress on the material of the shaft than that estimated from the greatest turning moment the engines appeared capable of exerting. A shaft, in the circumstances described, would have a particular period of angular oscillation, and if this was the same as any considerable variation in the load, the shaft would probably break, although apparently not overloaded. The load on the steel shell of the boilers was about 8,300 lbs. on the reduced section, or about 6,000 lbs. on the gross plate per square inch of section.

It was the opinion of Mr. Flannery that the endurance of the fire bars had probably limited the duration of the evaporation experiments referred to. The author was not present, and could not therefore say, but he did not believe that was the case, as the fire bars would generally stand for a number of runs at full speed, and it was very unusual to have them interfere with an experiment. With regard to running engines at high speed, it was not customary to give large marine engines sufficient bearing surface to allow them to continue to run very fast, and the size of ports and passages were also not adapted to any considerable increase of speed without showing loss on the diagrams.

On the Manufacture of Magnets.—By G. Trouvé.—The author communicates the method by which he has attained success in the making of permanent magnets. Three points are observed: (1) To obtain the best steel, (2) to determine the degree of temper, (3) to select the most simple and practical process of magnetization. The tempering is effected in a gas muffle at a constant temperature. The magnetization results from placing the bars in two juxtaposed solenoids, the magnetic circuit being closed by two plates of soft iron; the current is obtained from a battery of 6 Wollaston elements. Magnets so made will carry twelve to fourteen times their weight; if the magnet is bent to a horseshoe, the charge is quadrupled, that is to say, the magnet will carry forty-eight to fifty-six times its weight. Magnets furnished with bobbins having 120 meters of No. 36 wire (0.1 millimeter in diameter) and a resistance of 240 ohms make very powerful Bell telephones.—*Comptes Rendus de l'Academie des Sciences.*

ON THE STRENGTH OF WROUGHT-IRON BRIDGE MEMBERS; INCLUDING STRUTS, COLUMNS, SEMI-COLUMNS, BEAMS, &c.

By S. W. ROBINSON, C. E., Prof. Mech. Eng. in the Ohio State University, Columbus, Ohio; Member of the Ohio State Board for Inspection of Railroads and Bridges.

Written to accompany Report to the Hon. H. SABINE, Commissioner of Railroads and Telegraphs.

(Read before the American Society of Mechanical Engineers.)

II.

GENERAL APPLICABILITY.

All expressions for the moment M, from equation (17) to (79) inclusive are applicable in equations (6) and (8), the former giving the strain t_1, due to the bending moment alone, and the latter the total or max. strain t, in the section considered. Any section throughout the length of the beam may be examined for strain, but usually the section under maximum strain is the one to be attended to.

It is to be observed that the strain due to M, is not in any case of the existence of T to be taken as the only strain. That due to T directly, according to (7), is to be included. For instance, points of contrary flexure are usually regarded as locating sections which are free from all strain except "transverse shear." But in all cases of points of contrary flexure mentioned above there will be not only transverse shear, but direct longitudinal tension or compression also. Hence the resultant stress will in these cases be diagonal in direction.

It is to be observed that T and t have been universally employed both for tension and compression. This was simply for convenience. In application, the proper distinction is to be made.

In cases where T is compressive in the above expressions for M, the beam may be regarded as acting partly like a beam and partly like a column, and we may vary the intensity of action from one almost purely of the first kind to one of the second kind.

ORDINARY BEAM FORMULAS

But at the limit $T=o$ some difficulty is experienced as, for instance (77) reduces to $\frac{0}{0}$ for $T=o$. But let the tangent be developed into a series, thus:

$$M_{max} = \frac{Q}{2n}\text{tang } n\frac{l}{4} =$$

$$\frac{Q}{2n}\left\{n\frac{l}{4}+\frac{n^3}{3}\left(\frac{l}{4}\right)^3+\frac{2n^5}{15}\left(\frac{l}{4}\right)^5+\&c\right\}$$

$$=\frac{Q}{2}\left\{\frac{l}{4}+\frac{n^2}{3}\left(\frac{l}{4}\right)^3+\&c\right\} \quad . \quad . \quad (80).$$

or

$$M_{max}=\frac{Ql}{8} \text{ for } n=o \quad . \quad . \quad . \quad (81).$$

This is the same expression for the maximum moment of flexure as we find given in works on the strength of beams for the case of a beam fixed at the ends and a load Q at the middle; conditions identical with (81) where $T=o$.

Similarly, (78), by expanding the $\cot n\frac{l}{2}$ gives

$$M_{max}=P_1 r=-\frac{w}{n^2}+\frac{wl^2}{12}+\frac{wn^2l^4}{720}+\&c+\frac{w}{n^2}$$

$$=+\frac{wl^2}{12} \text{ for } n=o \quad . \quad . \quad . \quad (82).$$

which is also the same as given for this case.

Making use of the expanded series for e^x, expressions (72) and (73) will reduce also to (81) and (82). The other expressions of moment above may be likewise reduced for $T=o$, to the ordinary well-known formulas.

Thus it appears that the above equations and formulas are general, and the possibility of reducing the same to the ordinary expressions for simple cases corroborates all the circuitous mathematical work.

FORMULAS FOR COLUMNS, PILLARS, STRUTS, ETC.

In cases where the force T predomin-

ates, the piece acts mostly like a column. Passing to the limit by making the transverse forces zero, we have form (24):

$$\frac{-Ty_1-Pl}{\text{tang } nl}=\frac{P}{n}$$

or

$$=\frac{Ty_1}{\text{tang } nl}=0$$

if $P=o$. But as we propose that T shall be the predominating force applied to the piece, then $T\,y_1$ cannot be zero unless $y_1=o$, and the latter cannot be assured since T may produce considerable deflection to one side or the other. Theoretically there would be no deflection, but we know that practically there would be a very appreciable deflection for a piece of ten to twenty or more diameters in length before rupture; hence, we must treat the case as providing for a deflection, y_1.

Hence it appears that the denominator must be infinite, which requires that

$$nl=\frac{\pi}{2}$$

or, since $n^2=T\div\varepsilon I$

$$\frac{T}{\varepsilon I}=\frac{\pi^2}{4l^2} \quad . \quad . \quad . \quad (83).$$

for a column fixed at one end and free at the other, as in Fig. 17.

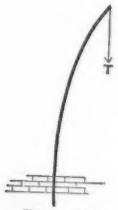

Figure 17.

The same result is obtained from (31). This expression (83) is given in several works, and produced in different direct ways.

Equation (49), for $Q=o$, requires, for like reasons, that tang $n\frac{l}{2}=\infty$

or

$$n\frac{l}{2}=\frac{\pi}{2}$$

$$\therefore \quad \frac{T}{\varepsilon I}=\frac{\pi^2}{l^2} \quad . \quad . \quad . \quad (84).$$

This expression is also obtained from (58), and is for columns having rounded ends; or, more specifically, pointed bearings. This expression is also given in several works on the strength of beams

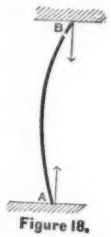

Figure 18.

obtained usually by a direct process. By some the column is said to have rounded ends for this formula, while others do not say whether the ends should be rounded, flat, or otherwise; that is, whether the end of the column is fixed or hinged, &c. The manner of holding the end, however, is not a matter of indifference, equation (49) showing that (84) is for rounded or hinged ends as shown in Fig. 18.

Equation (77) or (78), for Q or $wn=o$ and T not, requires that

$$\text{tang}\frac{l}{4}=\infty \text{ or } n\frac{l}{4}=\frac{\pi}{2}$$

or that

$$\cot n\frac{l}{2}=\infty \text{ or } n\frac{l}{2}=\pi$$

either of which gives

$$\frac{T}{\varepsilon I}=\frac{4\pi^2}{l^2} \quad . \quad . \quad . \quad (85).$$

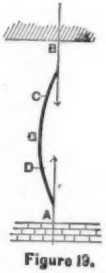

Figure 19.

which is for columns fixed at both ends, or sometimes considered as having flat ends, as shown in Fig. 19.

This expression is not common in published works as applying to columns with fixed ends. An article of much merit which appeared in VAN NOSTRAND'S ENG. MAG., Vol. 17, p. 257, gives the

above equation (84), but does not state whether the columns are for round or flat ends except as indicated by a figure like Fig. 18. But later articles in VAN NOSTRAND'S MAG. by J. D. Crehore and Prof. Baldwin give both (84) and (85,) also (87) with statement of end conditions.

To obtain the expression for a column having one end fixed and the other end rounded, we naturally turn to case XI, above. But the deduction of a satisfactory expression from this is difficult. However, reasoning from analogy, compare Figs. 18 and 19 with their expressions. Thus,

$$\left.\begin{array}{l}\text{For Fig. 18, } nl=\pi\\ \text{For Fig. 19, } nl=2\pi\end{array}\right\} \quad . \quad . \quad . \quad (86).$$

Observing that the curves are sinusoids—see eqs. (22) for $h=o$ and (47.)—we find AB, Fig. 18, like c D. Fig. 19, both being complete flattened sinusoids. But the middle of the beam at G, Fig. 19, is parallel to the same at A or B; from which we conclude that

$$AD+BC+CD=2 \text{ sinusoids.}$$

Hence the number of sinusoids in the curves Figs. 18 and 19 agree with the coefficients to π in the respective expressions (86).

Now for a column which has one end flat and the other rounded, the part CDA, Fig. 19, would represent it nearly, the chief point of difference being in the fact that C, Fig. 19, is not quite on the line of action AB. Assuming that the point D does not essentially change position as C is brought to the action line, then we can safely adopt $\frac{3}{2}$ as the coefficient to π for the expression corresponding to (86), because in Fig. 18 we have one sinusoid; in B, Fig. 19, two sinusoids, and in CDA $\frac{3}{2}$ sinusoids.

Now the point C, Fig. 19, is a point of contrary flexure, or a point where the bending moment is zero, and hence the condition of CDA would not be altered by cutting off the column at c and rounding it, thus giving, in CDA, a column with the desired conditions.
Hence the expression

$$nl=\frac{3}{2}\pi$$

or

$$\frac{T}{\varepsilon I}=\frac{9}{4}\frac{\pi^2}{l^2} \quad . \quad . \quad . \quad (87).$$

for the case of columns having one end flat or fixed, and the other rounded or pointed.

The conditions are shown in Fig. 20,

Figure 20.

where A is fixed, and B rounded or hinged.

These formulas, (83) and (84), have been proposed for use by some writers, without modification, though apparently with much hesitation and doubt; others have declared them wholly unfit. It appears that Hodgkinson compared (84) in the form

$$T=\varepsilon\pi^2\frac{I}{l^2}=\frac{\varepsilon\pi^2}{64}\cdot\frac{d^4}{l^2}$$

where

$$I=\frac{\pi d^4}{64}$$

with his experimental results, but finding it discordant, only used it as suggestive of form for an expression for columns, thus:

$$W=C\frac{d^n}{l^m} \quad . \quad . \quad . \quad (88).$$

where W = wt. sustained, d = diameter, l length; and with C, m and n, constants to be determined by experiment. The differences made manifest by Hodgkinson's experiments between (84) and (88), as to the values of m and n, are such, except by further inquiry, as to destroy all confidence in (84) as a formula to use directly in calculating the strength of columns. The same is true of (85) and (87.) This is unfortunate as regards one important fact, viz: (84) contains the moment of inertia of the cross section instead of merely the diameter, so that (84) may be applied to any form of section, such as round, square, oblong, I and H sections &c., while (88) must be pre-

pared for each characteristic section. Also, (84) contains one important factor, ε, which qualifies that formula for at least this one property of the material going into the column. But evidently there is, at least, one other property which is equally important, viz: the crushing resistance; or, if preferred, the elastic limit, neither of which appears in (84). Hence, though (84) has some valuable features, it must be pronounced sadly deficient as a general formula, except possibly for very long columns, such as would fail by simply springing out, without breaking. But evidently columns relatively short, though longer than "blocks," and such as would fail partly by crushing and partly by springing, should have a formula containing both the coefficient of elasticity ε, and some modulus of resistance, such as t, for compression or tension, usually the former. Again still shorter columns or "blocks" would only require the modulus of crushing resistance.

Let Fig. 21 represent a portion of a

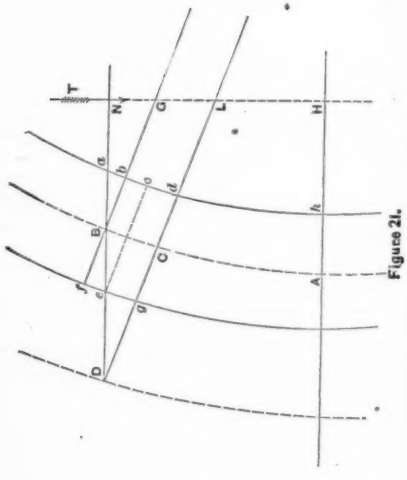

Figure 21.

column under strain from the action of T, the strain being partly compressive and partly flexural.

To formulate the relations between these actions, suppose AB, Fig. 21, to be a unit's portion of a column near its middle, which is sprung out of its original axial position NGH, to AC, the original axial fiber HN being now shortened and deflected to AC. The section dg was before flexure on the line ac, the intersection at D being a point on the real neutral axis of the column. The distance along DC to the intersection with AH, is the radius of curvature ρ. Draw BG parallel to DC, intersecting AH. The distance from this latter point to B is equal to ρ, because of the parallelism of BG and DC.

Then, according to (5), taking the line GH as the axial line of the column previous to flexure, and the line along which the compressive force T still acts, we will have the applied bending moment equal

$$\frac{\varepsilon I_1}{\rho} = T y_1 \quad . \quad . \quad . \quad (89).$$

where I is the principal moment of inertia, or monent of intertia, for an axis perpendicular to the plane of the curve ACB, at the center of gravity B of the section considered; y_1=BN, the moment arm of T, and ε, the coefficient of elasticity.

This supposes that the ends of the column remain in the same position while the body deflects to one side or the other, as it will do in columns of considerable length before rupture—a fact known by experience. Hence the column is represented as deflected to one side in order that the conditions of the problem may coincide with experience. Equation (89) applies evidently to any unit length of the column if y_1 be supposed to be the ordinate to the axis for that point, and this without regard to conditions outside of that unit portion. It applies whether the column be fixed, rounded or otherwise, at the end, or even if the column be bellied. But the ordinate y_1 will be different for intermediate portions as depending on form of body of column as to taper, belly, &c., from which it appears that a bellied column will support differently from a prismatic one. Our analysis applies to the latter. The ordinate is marked y_1 instead of y, to distinguish it as the maximum ordinate to the axial curve of the column; that ordinate being chosen because the moment of T is here greatest, and hence may denote the ordinate at the rupturing point of the column.

In (89) the 2d member is the applied moment, and is that which simply bends the column. The 1st member is the moment of resistance to this bending,

and represented in the figure by the triangle B*ab* of compression, and B*ef* of tension.

To obtain another equation take the origin of moments at D on the real neutral axis. Then the total moment of resistance is

$$\frac{\varepsilon I}{\rho}=T\,(y_1+BD) \quad . \quad . \quad . \quad (90).$$

where I is now the moment of inertia of the section at B for an axis at D; (y_1+BD) the moment arm of T; and ρ the same as in (89), as already stated. By a well-known formula, we have

$$I=I_1+K.\overline{BD}^2$$

B being the center of gravity of the section.

Introducing I, we have from (90),

$$\frac{\varepsilon I}{\rho}=\frac{\varepsilon I'}{\rho}+\frac{\varepsilon}{\rho}K\overline{BD}^2=Ty_1+T.BD$$

or, by (89)

$$\frac{\varepsilon}{\rho}K\overline{BD}^2=T.BD \quad . \quad . \quad . \quad (91).$$

But calling the shortening $BC=\lambda$, we have

$$\lambda : BD :: 1 : \rho$$

whence

$$\frac{BD}{\rho}=\lambda$$

and (91) becomes

$$\varepsilon K\lambda.BD=T.BD \quad . \quad . \quad . \quad (92).$$

But from the fundamental law of elasticity, see eq. (4½), observing that the shortening $BC=\lambda$ is for a unit length

$$T=\varepsilon K\lambda.$$

Combining this with (92), it becomes an identical equation which proves its correctness.

To determine the practical limitations of the ordinate y_1 let T be applied at E, Fig. 22, to a prism of unit's length. First, let the distance, BE, = the *principal radius of gyration*, $=k$. The point B is the center of gravity of the section at ED, or top of the prism. Now, as T is applied at E, let an equal and opposite force be applied at B, as shown. We thus have a couple acting to turn the top surface of the prism around B from the line EBD to FBH. If now we remove the counteracting force at B, while T is kept constant, the point, B, will fall by the compressive action of T to some point C. Call $BC=\lambda$. This will be found $=EF$, when $BE=k$. For we have, by taking moments at B,

$$Tk=\frac{\varepsilon I_1}{\rho};\ \text{also } I_1=Kk^2,\ \text{and } T=\varepsilon K\lambda$$

combining,

$$\frac{T}{\lambda}\frac{k^2}{\rho}=Tk \text{ or } k=\rho\lambda$$

whence

$$\rho : 1 :: k : \lambda, \text{ or : BC}$$

Hence $\lambda=BC=EF$ as proposed.

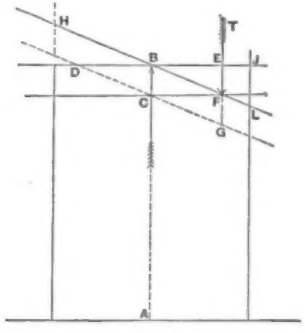

Figure 22.

Applying T at any other point, we have a different result, for suppose it at J. The compressive effect, BC, will be the same for the same value of T as long as it is the only compressive force. But the fall due to bending will be greater, even greater than JL, because the moment arm is greater.

Hence when a compressive force, T, is applied at the distance, k, from the center of the cross section of a prism, the displacement, EF, due to bending is exactly equal to the displacement, FG, or BC, due to direct compression.

Next suppose the neutral axis to be taken at a distance qk from B while T is applied at E. Then eq. (90) gives

$$T(k+qk)=\frac{\varepsilon I}{\rho}=\frac{\varepsilon I_1}{\rho}+\frac{\varepsilon}{\rho}K(qk)^2$$

or since $Tk=\frac{\varepsilon I_1}{\rho}$ . . . (92a).

$$Tqk=\frac{\varepsilon K}{\rho}q^2k^2$$

$$Tk=\frac{\varepsilon K}{\rho}qk^2 <= \text{ or } > \frac{T}{\rho\lambda}k.qk$$

where $\rho\lambda=k$ when T is applied at E, and $T=\varepsilon K\lambda$

Hence

Tk=applied moment

<=or> moment of resistance.

To make the first and second members equal, we must put $q=1$; and hence we must also have the neutral axis at a distance, k, from the center, B, of the section, for the condition that the column is indifferent as to whether it deflects more or less. If it deflects more, y_1 or BE, becomes $>k$, and $BD<k$, or $q<1$, and then the greater the displacement the greater is $BE=y_1$, and hence the greater the deflection of the column, Fig. 21, beyond $y_1=BE=k$, the greater is its tendency to deflect, because the applied moment becomes greater than the moment of resistance. But, on the other hand, when the ordinate, $y_1=BE$ is less than k, BD is greater than k, $q>1$, and the column is stable. It appears, therefore, that Tk is the greatest admissible applied moment. We, therefore, follow the condition that in eqs. (90) to (92), $BD=y_1$.

Now in Fig. 21, we have

$$ad : (BD+d_1) :: 1 : \rho$$

or
$$ad=\frac{y_1+d_1}{\rho}$$

and
$$\varepsilon.ad=t$$

where $BD=y_1$, $d_1=Ba$=distance from the center of gravity of the section to where the fiber ruptures by compression, and t the greatest admissible compressive force per square inch for a unit's length of prism.

$$\therefore \quad ad=\frac{t}{\varepsilon}=\frac{y_1+d_1}{\rho} \quad . \quad . \quad . \quad (93).$$

But combining

$$\frac{\varepsilon I_1}{\rho}=Ty_1 \quad . \quad . \quad . \quad (94).$$

with equations (84), (85), and (87), observing that I is the same as I_1 in (94), we get for columns

$$\left.\begin{array}{ll} \text{with round ends...} & \frac{1}{\rho}=y_1\frac{\pi^2}{l^2} \\ \text{with flat ends.....} & \frac{1}{\rho}=4y_1\frac{\pi^2}{l^2} \\ \text{with round and flat.} & \frac{1}{\rho}=\frac{9}{4}y_1\frac{\pi^2}{l^2} \\ \text{with fixed and free.} & \frac{1}{\rho}=\frac{1}{4}y_1\frac{\pi^2}{l^2} \end{array}\right\} \quad . \quad . \quad (95).$$

Combining these with (93), we obtain

$$\text{for round ends} \quad \frac{tl^2}{\varepsilon\pi^2}=y_1^2+d_1y_1$$

or, solving for y_1

$$y_1=\frac{d_1}{2}\left(\sqrt{1+\frac{4tl^2}{\varepsilon d_1^2\pi^2}}-1\right) \quad . \quad (96).$$

But by eq. (8), the total compression on the critical fiber ah, Fig. 21, is

$$t=Ty_1\frac{d_1}{I_1}+\frac{T}{K} \quad . \quad . \quad . \quad (96a).$$

or, introducing y_1 from (96)

$$t=\frac{T}{I_1}\frac{d_1^2}{2}\left(\sqrt{1+\frac{4tl^2}{\pi^2\varepsilon d_1^2}}-1\right)+\frac{T}{K}$$

and the remaining equations in (95) may be treated similarly.

Solving for the load, T, borne by the column, we obtain the following general formulæ, viz.: for columns with

Round ends

$$T=\frac{tK}{1+\frac{Kd_1^2}{2I_1}\left(\sqrt{1+\frac{4tl^2}{\pi^2\varepsilon d_1^2}}-1\right)} \quad . \quad (97).$$

Flat ends

$$T=\frac{tK}{1+\frac{Kd_1^2}{2I_1}\left(\sqrt{1+\frac{tl^2}{\pi^2\varepsilon d_1^2}}-1\right)} \quad . \quad (98).$$

Flat and round

$$T=\frac{tK}{1+\frac{Kd_1^2}{2I_1}\left(\sqrt{1+\frac{16}{9}\frac{tl^2}{\pi^2\varepsilon d_1^2}}-1\right)} \quad . \quad (99).$$

Fixed and free

$$T=\frac{tK}{1+\frac{Kd_1^2}{2I_1}\left(\sqrt{1+\frac{16tl^2}{\pi^2\varepsilon d_1^2}}-1\right)} \quad . \quad (100).$$

We observe that in using the equations (84), (85) and (87), our object is to obtain a relation between ρ and y_1, as given by the curves of sines of the elastic axes of the respective columns, and not to employ them as the foundations for the general formulas, because the latter object is found in (93), (94) and (96a), which give the limiting ordinate, y_1 the moment of flexure, and the limiting strain respectively.

In these formulas t is the greatest admissible compression per square inch, or the modulus of crushing, for breaking

loads, K, the sectional area of the column at the middle, d_1 the distance from the center of gravity of the section to the fiber which ruptures first, I, the moment of inertia for an axis at the center of gravity of the section, and at right angles to the plane of the axial curve of the deflected column; l, the length of the column, and ε, the coefficient of elasticity of the material. Also we have

$$\frac{K}{I_1}=\frac{1}{k^2},$$

where k is the principal radius of gyration.

These formulas are readily adapted to special forms of section. For instance, for solid cylindrical columns with diameter$=2r$, (97) becomes, for rounded ends,

$$T=\frac{\pi r^2 t}{1+2\left(1+\frac{4tl^2}{\varepsilon r^2\pi^2}\right)^{\frac{1}{2}}-2}$$

But it is useless to make these adaptations since the present practice in designing bridge struts requires formulas which will be applicable to all conceivable forms of built struts or columns, such as combined I beams and channel bars, latticed channel bars, &c.

A Simplified or Approximate Expression.—In cases where the length of the column divided by the diameter is relatively small, the radical part of the denominator may be developed into a series, by aid of the binomial theorem, for a more convenient expression. In doing this, let us adopt one expression in common for (97), (98) and (99), by placing the numerical coefficient under the radical equal to a, leaving the general expression for it to be determined later. Also, introducing the principal radius of gyration, k, we have

$$T=\frac{tK}{1+\frac{d_1^2}{2k^2}\left\{\left(1+\frac{atl^2}{\varepsilon d_1^2\pi^2}\right)^{\frac{1}{2}}-1\right\}} \quad . \quad (101).$$

Now by placing

$$v=\frac{atl^2}{4\pi^2\varepsilon k^2} \text{ and } z=\frac{k^2}{d_1^2} \quad . \quad . \quad . \quad (102).$$

we obtain, with the developed denominator,

$$T=\frac{tK}{1+v-v^2z+2v^2z^2-5v^4z^3+\&c} \quad . \quad (103).$$

Now, in examining this equation, we find that for very short columns, we only need to retain $1+v$, and the formula then appears independent of d_1, which indicates reasonably enough that a column failing almost entirely by crushing, might have d_1 reckoned in one direction about as well as in another, for the distance from the center of gravity of the failing cross section to the point of incipient rupture; that is, for crushing on all sides the direction for d_1 is indeterminate, and d_1 as such, is not required.

For lengths somewhat greater, additional terms will be required for the denominator; but eventually, for considerably increased length, the denominator converges too slowly, requiring us to fall back upon the undeveloped denominator.

Special Simple Formula for Unusually Long Columns.—But at length we find a limit where the expression (101) will give the same value for the supporting power of the column as the corresponding simpler expression of (84), (85) or (87), which contain no factor for crushing resistance. Beyond this limit, the latter may be employed without hesitation, and failure will be expected to occur by springing instead of crushing.

To obtain an idea of the position of this limit, the ratio of length to diameter has been computed for solid cylindrical wrought iron columns and found to be for columns with

ends rounded ..... $\frac{l}{d}=36.2$

ends flat..... $\frac{l}{d}=72.4$

ends round and flat.... $\frac{l}{d}=54.6$

For square, hollow and open-built columns, the ratios are still greater.

Hence, these last named formulas, previously rejected, now return to us again for application to very long columns. For this purpose, fortunately, the objection to these formulas, previously stated, of the absence of the modulus of crushing, t, has no weight, because failure occurs by springing and not by crushing.

Criterion for Long Column Formula.—Evidently this limit is to be found by placing the values of the load T as given

by the two formulas equal to each other, and then solving for the

$$\text{limiting ratio}\frac{l}{d}$$

For rounded ends we have therefore

$$T=\varepsilon I\frac{\pi^2}{l^2}=\frac{tK}{1+\frac{Kd_1^2}{2I_1}\left(\sqrt{1+\frac{4tl^2}{\varepsilon\pi^2 d_1^2}}-1\right)} \quad . \quad (104).$$

To avoid multiplying the solutions, it is desirable to obtain one which includes the three conditions of ends rounded, ends flat, ends round and flat. This may be done by aid of a, which is $=4$ in (104). To adapt a coefficient for the first member we observe that $\frac{4}{a}$ is sufficient. To provide for the fact that d_1 and the radius of gyration k do not bear a constant ratio to each other, for all columns, assume

$$\frac{d_1}{k}=\beta$$

Then, observing that k^2 is equal, the moment of inertia divided by the section, we have by introducing a and β,

$$\frac{T}{K}=\frac{4\varepsilon\pi^2}{a\beta^2}\frac{d_1^2}{l^2}=\frac{t}{1+\frac{\beta^2}{2}\sqrt{1+\frac{atl^2}{\varepsilon\pi^2 d_1^2}}-\frac{\beta^2}{2}} \quad . \quad (105).$$

an expression in which the first member, placed equal $\frac{T}{K}$, will give any one of the expressions (84), (85), or (87), by introducing proper values of a and β. Also the second member of which will give (97), (98) or (99), with like attention to a and β.

Clearing of fractions we obtain

$$\sqrt{1+\frac{atl^2}{\varepsilon\pi^2 d_1^2}}=\left(1-\frac{2}{\beta^2}\right)+\frac{atl^2}{2\varepsilon\pi^2 d_1^2}$$

Squaring, and solving for $\frac{l^2}{d_1^2}$, we obtain

$$\frac{l^2}{d_1^2}=\frac{4}{a\beta}\left(1+\frac{1}{\beta}\right)\frac{\varepsilon\pi^2}{t} \quad . \quad . \quad . \quad (106).$$

a formula which serves as the criterion of applicability for $\frac{T}{K}=$ first part; or= second part of (105), as a formula for extremely great or for ordinary lengths, respectively.

When the axis for the moment of inertia is an axis of symmetry for the cross section, we have $2d_1=d=$ whole diameter of column. This is the thickness in this case in the direction of the plane of the curve of flexure of the column, and is the extreme thickness, whether the column be solid, hollow or open-built.

When the cross section is thus symmetrical, we have

$$\frac{l}{2d_1}=\frac{l}{d}=\pi\left\{\frac{1}{a\beta}\left(1+\frac{1}{\beta}\right)\frac{\varepsilon}{t}\right\}^{\frac{1}{2}} \quad . \quad . \quad (107).$$

As an example, in cylindrical columns, d_1 is the radius of the column, and k the radius of gyration.

$$\frac{d_1}{k}=2=\beta$$

and for flat ends,

$$a=1$$

Taking $\varepsilon=28{,}000{,}000$ and $t=40{,}000$, we obtain from (106) or (107) the value 72.4, the same as given above.

FORMULA FOR END CONDITIONS.

Let us now determine a general expression for the qualifying factor a, an expression which shall duly qualify the general column formulas (105) for the case of fixed ends, flat ends, or of rounded ends (pointed); or for pin ends with large pins under friction, or for flat ends which are narrower than the middle section, or for one of these forms at one end, and another at the other, &c.

From (97), (98), and (99), we already have

$$\left.\begin{array}{l}\text{For pointed ends}\ldots\ldots a=4, m=o\\ \text{For fixed ends}\ldots\ldots a=1, m=2\\ \text{For pointed and fixed.} a=\frac{16}{9}, m=1\end{array}\right\} \quad (108)$$

where m stands for the moment effect of the end conditions preventing lateral displacement of column at the middle. In Fig. 23, the pointed ends a and b offer no hindrance to the side deflection of the middle, m, of the column, hence in (108) m is zero. But at c and d, there is the stiffeners of the fixed ends, to prevent the middle from deflecting; and hence for this case in (108), $m=2$, sim-

ilarly for the third part of Fig. 23, and of (108) $m=1$.

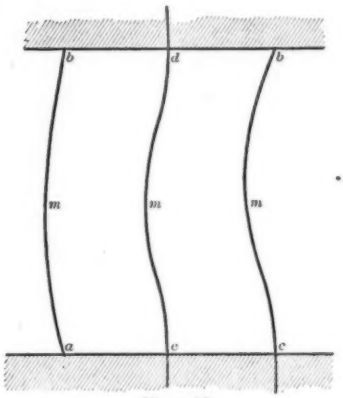

Figure 23.

Fixed Ends.—Now if we consider the stiffness at a fixed end as equivalent to a moment $p\frac{l}{2}$ preventing lateral displacement of the middle, we may combine it with the end and middle moments due to T as follows:

The expression Tk, see (92a), appears to be the max. admissible moment of flexure at the middle. Using a similar expression for the end, we have

$$p\frac{l}{2}=\mathrm{T}k'$$

where k' is different from k when the fixed end of column differs in size and form from the middle section. Designating the latter, or radius of gyration of the middle section by k_m, and of the end surfaces or sections by k_c and k_d, we will have the total counteracting moment

$$\left(\frac{p\frac{l}{2}}{\mathrm{T}k_m}\right)=\frac{\mathrm{T}k_c+\mathrm{T}k_d}{\mathrm{T}k_m}=\frac{k_c+k_d}{k_m} \quad . \quad (109).$$

an expression which admits of any size of end sections less than the middle section. Even the ends c and d may differ. Now we find by trial that a may be obtained from the expression

$$a=\frac{4}{\left(1+\frac{k_c+k_d}{2k_m}\right)^2} \quad . \quad . \quad . \quad (110).$$

for ends actually fixed.

To illustrate, if k_c and k_d are zero, $a=4$ as for pointed ends, thus producing (97) from (105) as regards qualifying with respect to a. Again put $k_c=k_d=k_m$ and $a=1$, as for (98). Finally for $k_c=k_m$ and $k_d=o$, we have $a=\frac{1}{9}\frac{6}{}$ as required in (99).

But suppose the ends are only half as large as the middle and similar. Then $k_c=k_d=\frac{1}{2}k_m$ and a becomes $\frac{1}{9}\frac{6}{}$, the same as for one end pointed and the other as large as the middle, and fixed.

To distinguish between actual fixed ends, and flat ends, it appears that in the former the column cannot fail at the end without a similar failure at the middle. According to the conclusion BD=y_1 in Fig. 22, there must be some tension in the section both at the middle and end of column. That is; the neutral axis is at D when rupture takes place, and outside of D there will be tension. Hence when the end sections are cut as in flat ends; that tension cannot act, and the column is weaker than for actual fixed ends. It appears therefore that the neutral axis should not approach the axis of the flat ends nearer than the outermost surface of the column. As the neutral axis is at D, where BD=the radius of gyration, k; we may obtain in flat ends, the equivalent to fixed ends, by making the radius of gyration of the flat end surface of the column equal to d_1. This necessitates enlarged, or disc-like ends.

Equation (110) is supposed to meet the real case of fixed ends. To adapt it to the case of flat ends, it is only necessary to replace k_m by d_1: because the same value of a would then be given by (110) thus modified for k_c and $k_d=d_1$, as would now be given by (110), as above, for k_c and $k_d=k_m$. That would make the radius of gyration of the flat end bearing $=d_1$ and the neutral axis could not approach the axis at the end of the column nearer than d_1. Then there would be no tendency to tension in the flat ends, as well as no power to resist it.

Again in built columns it is often the case that thickening pieces are laid on at the ends, thus making the area of the flat ends greater than the sectional area at the middle. From analogy with the usual relation of k and K in sections of beams, it appears that the radii of gyration k_c k_d and d_1, should each be

multiplied by the square root of the proper section.

Hence, to include the two modifications of (110) now considered, it appears that the equation (110) should be written.

$$a=\frac{4}{\left(1+\frac{k_c\sqrt{\mathrm{K}_c}+k_d\sqrt{\mathrm{K}_d}}{2d_1\sqrt{\mathrm{K}_m}}\right)^2} \quad . \quad (110a).$$

The meaning of the subscripts is indicated in Fig. 23.

When the end surfaces or sections in every way equal the middle section of the column, the capitals, K, divide out and disappear, also when the ends are actually fixed d_1 should change to k_m and thence (110a) returns to (110).

When the flat ends are unenlarged, either in diameter or section, the capitals disappear; but d_1 remains and the denominator of (110a) is smaller than of (110), which makes the strength of columns with unenlarged flat ends less than that for fixed ends, which is evidently as it should be.

In no case should (110a) ever be used when the denominator gives a value greater than the denominator of (110).

Pin Bearings.—The pin bearing for the end mounting of a column is usually considered as acting in effect like a knife-edge terminal to the column. But it is evident at once that the friction of the pin must cause the column to act as though the end had some breadth which is appreciable, and hence not to be ignored. To provide for this, let r be the radius of the pin, and f the coefficient of friction.

Then, as the column bows to one side or the other, the point of bearing of the hole upon the pin will be thrown to one side a distance ox Fig. 24, which distance is greater or less according to the value of f. Now as is well known, the value of ox is

$$ox=fr.$$

By Fig. 23, this may be designated as fr_a or fr_b, as to whether it is expressed for one pin or the other.

In moment effect this is to be treated as an arm for T, so that the Tfr here, corresponds with Tk in respect to the fixed end. Hence, according to (109), we may write

$$\frac{\left(p,\frac{l}{2}\right)}{\mathrm{T}k_m}=\frac{fr_a+fr_b}{k_m}$$

a quantity which is of the same kind as (109) and may be incorporated in (110a) thus

$$a=\frac{4}{\left(1+\frac{k_c\sqrt{\mathrm{K}_c}+k_d\sqrt{\mathrm{K}_d}}{2d_1\sqrt{\mathrm{K}_m}}+f\frac{r_a+r_b}{2k_m}\right)^2} \quad . \quad . \quad . \quad (111).$$

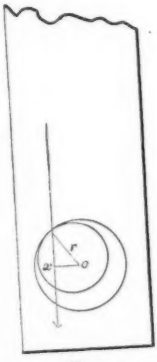

Figure 24.

In bridges, the pin is of considerable size, and the coefficient of friction quite high; probably between f=.2, to .3 for new bridges, and as high as .5 for older ones.

To illustrate, suppose the pin to be 4 inches in diameter at a and b. Then $fr_a = fr_b = .25\times 2''=.5''$, or$=\frac{1}{2}$ inch.

If the column be a solid cylinder, 8 inches in diameter at the middle $k_m=2''$; and observing that k_c and k_d are both zero here, we have

$$a=3.16$$

As ordinarily treated, this would be taken as 4 instead of 3.16.

Relative Positions of Pin Bearings.—Now with respect to knife-edged *vs.* pointed ends, it is evident that when the knife edges are in one plane, and so oriented as to admit of deflection in the same direction as when the ends are pointed, the supporting power of the column is the same for either form of end. But when the knife edges are in a plane at right angles to the plane of deflection for points, the strength of the column is

greater. This is with the understanding that the moment of inertia of the middle section is greater for an axis in one direction than in another. With a given cross-section of column the greatest strength with pin bearings is obtained by placing the knife edges parallel to the axis for that max. moment of inertia of that cross section. For deflections constrained by guides to be in the line of the knife edges, the strength is as for a column with flat ends. When a knife edge is placed at each end of a column, the strength is increased by placing the knife edges at right angles to each other. But for this hypothesis it is difficult to calculate the real strength of the column, or to determine the direction of the deflection.

To investigate this let us first take the first part of eq. (105). Replacing β by its value we have for the supporting power per square inch

$$\frac{T}{K}=\frac{4\varepsilon\pi^2 k^2}{a}\cdot\frac{}{l^2}=t$$

Let l be constant for a particular case. Then the strength is constant if $\frac{k^2}{a}$ is constant. If this ratio is variable the strength of the column is variable, for deflection in different directions as controlled by guides near the middle. For each position of the guides there may be noted a deflection, a radius of gyration, and a knife edge component.

According to considerations previously noted, the column is counteracted in its tendency to deflect by a moment Tk at the middle, and Tx, say, at the end; where x is the arm of the moment. Now for the middle cross section of the column; let the radius of gyration be found for a number of different directions within the circumference. Lay these off as in Fig. 25 where OE OG OH OK are 4 values for the 4 respective directions. Then trace the curve EG HKE. Any radius vector to this curve will be the value of k lying in the direction of that radius vector.

The point O represents the axis of the column. Let AB represent the knife edge bearing at one end of the column, its effective length being laid off to the same scale as k. The effective length should be less than the actual length, for the same reason that k is less than d_1; that is, the pin bearing will crush for some distance, corresponding to d_1-k.

Now for a constrained deflection toward OA, lay off x=OA as the moment arm. This added to k gives OK+OA=OL. Again for a constrained deflection toward OJ the knife-edge component of

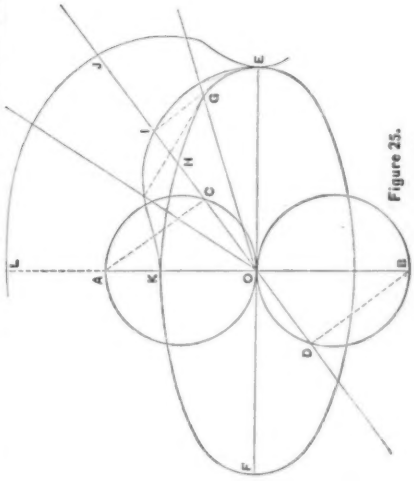

Figure 25.

OA will be OC=OA projected upon OJ. C is a point in a circle OCA giving all components. The curve EGHK should be treated in a similar way for finding the components. For instance project upon OJ by the tangent from the curve EGH giving I one point in the curve EIK. Any radius vector to this curve gives the k component for that direction. Now add OC and OI giving OJ. J is another point in the curve EJL, a radius rector to which represents the resultant moment arm for the moment counteracting deflection in the direction of radius vector considered.

We should yet add the counteracting effect due to the opposite end of the column. Another curve outside of EJL including the three; the middle and the two ends. Then the minimum radius vector to that curve will be in the direction in which the column will deflect when unconstrained.

In practice it will be found that for almost every possible case, the minimum radius vector will lie in the direction of OA or OE so that the column will but

rarely deflect diagonally. The minimum load value of the column will be found by computing for the direction of deflection above indicated. If the deflection takes in the direction of the pins, a is to be found from (111) by ignoring r and taking k_c and k_d as equal the respective values of OA.

If the knife edges be at right angles, the curve, Fig. 25, obtained will be found to have minimum ordinates in the directions of the pins or knife edges.

Fig. 25 applies especially for the long column formula. For shorter columns proper to the formula (101) we will probably find less decided differences between max. and min. radii vectors to Fig. 25 until for short blocks the differences are either indeterminate or indistinguishable.

Open Column Stays.—Open built columns are occasionally deficient in trussing or latticing. For instance, two 8″ channel bars may be joined by $\frac{3}{8}$″ plates 10″ square riveted on in such a way that the plane sides of the bars are back to back with an intervening space of 6 or 7 inches, the flanges being turned outward. Then if the plates are placed 3′ 10′ apart along the bars, the column thus constructed will probably be deficient in cross stays. In such case the column will fail to carry the load determined for it by the usual formula and actual moment of inertia of the cross section. To illustrate, the failure may be as shown in Fig. 26 as indeed experiment

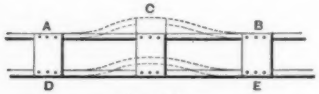

Figure 26.

has given, the parts ABC acting like little columns with fixed ends. It is proposed to find the length of a column made o the same plates fully stayed, which will be equivalent in strength to the actual part AB with its deficient stays. Let l'= AB less the width of the stay C; l''= length of equivalent full-stayed column. Then if the parts AB and DE are regarded as small columns with fixed ends, we have by applying the simple long-column formula for the purpose of securing a simple correction

$$T=4\varepsilon I'\frac{\pi^2}{l''^2}=4\varepsilon I_1\frac{\pi^2}{l'^2}$$

where I′ is the sum obtained by finding the least moment of inertia of each of the parts AB DE &c. taken separately and added; and I_1 the least moment of inertia of the entire section of the column obtained in the usual way as such. In this section, the staying is not counted. There the square of the ratio obtained by dividing the equivalent full-stayed column length by the actual column length supposed insufficiently stayed is obtained by solving the above for l''^2 and dividing by l^2, giving

$$\left(\frac{l''}{l}\right)^2=\frac{I_1}{I'}\left(\frac{l'}{l}\right) \quad . \quad . \quad . \quad (112).$$

This forms a co-efficient for l^2 in any of the column formulas. For instance, if $\frac{I_1}{I'}\left(\frac{l'}{l}\right)^2=3$, then in formulas (97), (98), or (99), &c., l^2 therein, should be multiplied by 3, when (112) is less than 1 it is not to be applied. This fact indicates that the column is sufficiently stayed.

Stay Spacing.—In the last statement we have the suggestion of a formula for determining the spacing in built columns. That is to say, placing the 2d member of (112)=1, and solving for l', we have twice the greatest allowable space between stay points in open-built columns, or,

Greatest admissible space between stay points

$$\frac{l'}{2}=\frac{l}{2}\sqrt{\frac{I'}{I_1}} \quad . \quad . \quad . \quad . \quad (113).$$

Thus, if a column be built of 4 angle bars L $\frac{1}{2}$″×3″, situated as forming the corners of a 12″ square in a column 20′ long, the greatest inter-stay space will be about 16 inches. As an example from actual experiment, column No. 33 of G. Bouscaren's report of experiments to the A. S. C. E. gives the spacing of the open column at 18 inches, and it failed by buckling between the lattices. The spacing, according to the formula (113), should not have exceeded about 14 inches, and hence it should have failed, as observed.

As to the space in open columns of different construction, it is evident that AB=l' is twice the space in Fig. 26, and in the first part of Fig. 27. But in the

second part of the latter, AB=l' is to be counted as the space where the cross and diagonal staying together serves to fix the ends of the parts, AB.

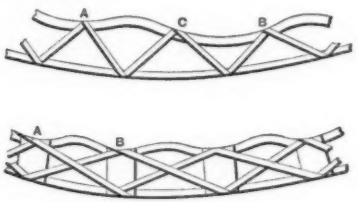

Figure 27.

PRACTICAL FORMULAS FOR COLUMNS.

Collecting the formulas which are useful in practice, we have:

For ordinary columns

$$\frac{T}{K}=\frac{t}{1+\frac{d_1^2}{2k^2}\left(\sqrt{1+\frac{atl^2}{\varepsilon\pi^2 d_1^2}}-1\right)} \quad . \quad . \quad (114).$$

For very long columns,

$$\frac{T}{K}=\frac{4}{a}\varepsilon\pi^2\frac{k^2}{l^2} \quad . \quad . \quad . \quad . \quad (115).$$

Criterion for applying 114 or 115

$$\frac{l}{2d_1}=\left\{\frac{\pi^2}{a\beta}\left(1+\frac{1}{\beta}\right)\frac{\varepsilon}{t}\right\}^{\frac{1}{2}} \quad . \quad . \quad (116).$$

$$\beta=\frac{d_1}{k}$$

For adapting to actual end conditions, see Fig. 23.

For flat ends,

$$a=\frac{4}{\left(1+\frac{k_c\sqrt{K_c}+k_d\sqrt{K_d}}{2d_1\sqrt{K_m}}+f'\frac{r_a+r_b}{2k_m}\right)^2} \quad . \quad . \quad . \quad (117).$$

For *fixed* ends change d_1 to k_m

Co-efficient for l', in (114) and (115) for columns deficient in staying,

$$\left(\frac{l''}{l}\right)^2=\frac{I_1}{I'}\left(\frac{l'}{l}\right)^2 \quad . \quad . \quad . \quad (118).$$

Inter-stay spaces in open columns

$$\frac{l'}{2}=\frac{l}{2}\sqrt{\frac{I'}{I_1}} \quad . \quad . \quad . \quad (119).$$

where l'=AB in Figs. 26 and 27.

In these formulas,

T=load borne by the column.

t =compressive resistance per square inch corresponding to T.

K=cross section of column, K_m at middle; K_c and K_d at ends, either when flat or fixed, and when flat to be taken as the flat-end surface. Only for suporting members.

d_1=distance from the center of gravity of the section K_m to where the strain is t, and measured on the plane of the curve of flexure.

d =$^2d_1$ in symmetrical cross sections, and equal the diameter of the column reckoned parallel to plane of flexure.

k =radius of gyration of the cross section, k_m, least radius at the middle of column; k_c and k_d radii for the end sections for fixed ends; or of the end surface for flat ends, and to include supporting members only.

l = length of the column.

ε = coefficient of elasticity of the material composing the column.

f = coefficient of friction of a column on its pin bearing, =about .25 to .5.

r_a =radius of pin at one end of column.

r_b =radius of pin at the other end of column.

I_1 =least moment of inertia of the entire cross section of supporting members taken in their actual relative position, and exclusive of staying.

I′= aggregate moment of inertia obtained by finding the least moment of inertia of supporting members taken separately, and adding

$\frac{l}{2d_1}$ >than in (116), apply (115).

$\frac{l}{2d_1}$ <than in (116), apply (114).

a = 4 for columns terminated in points or knife edges.

a = 1 for columns terminated in fixed ends no smaller than the middle of column.

a = $\frac{1}{9}^6$ for columns having one end pointed and one end fixed and large as middle.

a = (117) in other cases.

OTHER FORMULAS.

The well-known formulas of Hodgkinson appear to be the first of substantial value, their constants being wholly em-

pirical. They were applicable to only a few forms of cross section, and entirely inadequate to the present engineering practice.

Lewis Gordon determined the constants for Tredgold's formula from the experiments of Hodgkinson, which, on account of its partly theoretical basis, was found to have a much wider range of applicability than Hodgkinson's formula. It came into quite general use, and was known as Gordon's formula. Rankine has displaced the least diameter, h, in this formula by the least radius of gyration, thus producing the most acceptable formula in use to-day.

The form of this expression is:

$$w = \frac{f\mathrm{K}}{1 + a\frac{l^2}{k^2}} \quad . \quad . \quad . \quad . \quad (120).$$

where f and a are empirical constants, the former being nearly the ultimate resistance to crushing; K and k, the cross section and its least radius of gyration respectively, at the middle of the column length, l. The values adopted by different engineers for f and of a differ greatly, even for a given material, so that in bridge specifications it is common to see the chosen values of f and a given. Besides this, it is impossible to determine values for f and a for any particular material, except by actual column experiments—a fact which practically limits the formula to a few materials, and even then to a comparatively few experiments. Hence, a general formula whose constants consist of such factors as the coefficient of elasticity, modulus of crushing, &c., seems, by reason of the plentitude of these factors, very desirable.

These facts have caused search for such theoretical formula. One of the first formulas of this kind appears to have been proposed by Reuleaux, and published in *Der Constructeur*, the same being quoted by Ritter in his translation by Sankey of *Bridges and Roofs*, in 1879. See *Sankey's Ritter*, p. 345. The first edition was published in 1862.

This expression for rounded ends is of the form

$$w = \frac{c\mathrm{K}}{1 + \frac{c\mathrm{K}l^2}{8\varepsilon\mathrm{I}}}$$

$$= \frac{c\mathrm{K}}{1 + \frac{c}{8\varepsilon}\frac{l^2}{k^2}} \quad . \quad . \quad . \quad (121).$$

where c = resistance to compression, proposed to be taken within the elastic limit for a safe load, ε the coefficient of elasticity. K l and k as before. The form of (121) is seen to be the same as (120). That is to say, the load for a given material is equal to a constant times the section; divided by 1 + a constant times the square of the ratio of the length to the radius of gyration of cross section

Notwithstanding the agreement in form, (121) has the advantage of applicability to all materials for which C and ε are known, for many of which (120) might not be known. Even if (121) should give discrepancies as it stands, an empirical qualifying coefficient could be found which would probably be nearly the same for one material as another.

An expression of the same form as (121) for round ends was proposed by Mr. J. D. Crehore, and published in Van Nostrands Mag. for Dec., 1879, under the title, "A New Rational Formula for Pillars." Mr. Crehore's formula however differed in displacing 8 by π^2 in the denominator of (121). To account for this, Reuleaux treated the axis of the deflected column as a circle arc, while Crehore showed it to be a sinusoid. Hence Crehore's formula for rounded ends was

$$w = \frac{\mathrm{CK}}{1 + \frac{\mathrm{C}l^2}{\pi^2\varepsilon k^2}} \quad . \quad . \quad . \quad (122).$$

An expression identical with this for columns with round ends was given in an article by E. Hatzel, translated for Van Nostrand's Eng. Mag. for Sept., 1877; except in Hatzel's formula the moment of inertia was taken for an axis at other points in the section than the center of gravity; whence the radius of gyration k would be other than the principal radius.

Also Crehore showed that for flat ends the 2d term of the denominator of (122) should be divided by 4, and for one flat and one rounded end it should be divided by $\frac{16}{9}$.

Crehore's formulas were reproduced in an elaborate article by Professor W.

Baldwin, and published in VAN NOSTRAND'S MAG. for May, 1880, with due credit to Mr. Crehore. But in the same article Professor Baldwin utterly abandons these formulas, and proceeds to the discussion of the Gordon formula containing the least thickness h, and of the Rankine formula containing the least radius of gyration k. Adopting the latter, its constants are determined from a great number of experiments cited, and for a variety of materials. But no attempt was made to generalize by retaining C and ε. The reason given is that ε is not constant in failing material, and at the point of rupture is unknown. This is to be admitted, especially in tension; but in compression the value remains very nearly constant for both cast and wrought iron. This is fortunate, because the chief factor concerned in the strength of columns is the compressive resistance. As the column fails by deflecting to one side, the material is most severely compressed at the concave side, and only a small portion of the cross section will be involved in incipient fracture. Existing results of experiment show that ε is practicably constant for compression of cast and wrought iron to near the failing point, where it drops suddenly from 10 to 20 per cent. Hence if a tenth of the section is effected by 15 per cent. reduction in ε, the resulting effect upon the whole section would be one and one-half per cent., a quantity which may be ignored in calculating the strength of columns. Hence we propose to compare the rational formulas with experiment before giving them up.

But before taking up numerical facts we observe that the Reuleaux, Hatzel, or Crehore formula differ from the rational formula (97) for rounded ends, believed to be given for the first time in this article, in the form of the denominator. To show that this difference is an important one, we have from Fig. 21

$$ab : b\mathrm{B} :: \mathrm{BA} : \rho$$

or

$$ab : d_1 :: 1 : \rho$$

Also $(4\frac{1}{2})$ gives $t_1 = \varepsilon . ab$

$$\therefore \quad \frac{1}{\rho} = \frac{ab}{d_1} = \frac{t_1}{\varepsilon d_1} = y_1 \frac{\pi^2}{l^2}$$

the last expression being found in the first of (95).

whence

$$y_1 = \frac{t_1 l^2}{\varepsilon d_1 \pi^2}$$

in which t_1 is the compressive force which is sufficient to cause the shortening ab and a force which is due to bending only. Combining this with (8) or (96 a) we have

$$t = \frac{\mathrm{T} t_1 l^2}{\varepsilon \mathrm{I}_1 \pi^2} + \frac{\mathrm{T}}{\mathrm{K}}$$

which, solved for T, gives for columns with rounded ends,

$$\mathrm{T} = \frac{t\mathrm{K}}{1 + \frac{t_1 \mathrm{K} l^2}{\pi^2 \varepsilon \mathrm{I}_1}} = \frac{t\mathrm{K}}{1 + \frac{t_1 l_2}{\pi^2 \varepsilon k^2}} \quad . \quad . \quad . \quad (123).$$

an expression identical with Crehore's, except that t_1 is not the compressive resistance; Fig. 21 showing that it is only a part of it. In this expression t is the total compression represented by ad, and hence for rupture the total compression $t=\mathrm{C}$ of Crehore's formula. But t_1, represented by ab, cannot be C, and hence some oversight appears to have been committed in making it C in formula (122). This explains the difference in form between (97) and (122); and gives sufficient cause for a disagreement of one or the other with experimental results.

In the developed denominator of (97) see (102) and (103), we have in the terms, $1+v$, (observing that $a=4$ for rounded ends) the identical denominator of (122). Hence to the remaining terms of the denominator of (103) we may look for the compensating quantity for the error $\mathrm{C}=t_1$. The same fault is common to the Reuleaux, and to the Hatzel formulas though in the former a compensating effect appears to be provided in the values compounded of C and ε for use; and in the latter in assuming the moment of inertia axis at or near the convex side of column.

Direct Comparison.—But the column formula problem is one which cannot be satisfactorily settled for practical men from a purely theoretical standpoint. I therefore offer the following comparison of column formulas. As the numerator

may be considered as practically the same in all, the denominators only are compared.

Hence for the denominator we have for the

Gordon formula, $1+a\frac{l^2}{h^2}$

For round ends.	Round and fixed.	Fixed.
$a=\frac{4}{3000}$	$\frac{2}{3000}$	$\frac{1}{3000}$

Rankine formula, $1+b\frac{l^2}{k^2}$

For round ends.	Round and fixed.	Fixed.
$b=\frac{4}{36000}$	$\frac{16}{9}\ \frac{1}{36000}$	$\frac{1}{36000}$

By G. Bouscaren, C.E., and adopted by Keystone Bridge Company.

For round ends.	Round and fixed.	Fixed.
$b=\frac{2}{36000}$	$\frac{3}{2}\ \frac{1}{36000}$	$\frac{1}{36000}$

Reuleaux formula, $1+\frac{Cl^2}{8\varepsilon k^2}$ round ends

Crehore formula, $1+q\frac{Cl^2}{\pi^2\varepsilon k^2}$

For round ends.	Round and fixed.	Fixed.
$q=$ 1	$\frac{1}{2.28}$	$\frac{1}{4}$

New formula, $1+\frac{d_1^2}{2k^2}\left(\sqrt{1+\frac{atl^2}{\pi^2\varepsilon d_1^3}}-1\right)$

For round ends.	Round and fixed.	Fixed.
$a=$ 4	$\frac{16}{9}$	1

From these expressions for the denominators, I have computed the results found in the following

TABLE OF VALUES OF DENOMINATORS FOR WROUGHT IRON.

Data.	End.	Gordon.	Rankine.	Bouscaren-Keystone.	Crehore.	New.
Solid cylinder.....	Rounded.....	5.8	7.4	4.2	9.28	5.20
l=10 ft.....	Round and flat.	3.4	3.84	3.4	4.66	3.74
Diam.=2 in.....	Flat.....	2.2	2.6	2.6	3.06	2.50
Bar 2×12 in.....	Rounded.....	5.8	5.8	3 4	7.19	4.06
	Round and flat.	3.4	3.13	2.8	3.75	3.06
l=10 ft.....	Flat.....	2.2	2.2	2.2	2.54	2.17
Hollow cylinder...	Rounded.....	4.00	1.26	1.13	1.33	1.30
Diam. 8 in. and 6 in.	Round and flat.	2.50	1.14	1.10	1.15	1.17
l=10 ft..... ...	Flat.....	1.75	1.06	1.06	1.08	1.08
Hollow Rectangle..	Rounded	3.70	2.49	1.74	2 92	2.16
8×15 in. ×6×13 in.	Round and flat.	2.35	1.65	1.56	1.85	1.74
l=30 ft.....	Flat.....	1.67	1.37	1.37	1.48	1.39
Means.....		3.231	2.828	2.213	3.353	2.464

For the last two columns t=C=40000, ε=23370000, and $\frac{t}{\varepsilon\pi^2}=\frac{1}{7000}$.

The first three formulas named in the table have been in quite general use, and yet the results shown in the table obtained from them differ in some cases nearly four hundred per cent., as for instance for the Gordon formula applied to hollow cylinders.

Results by the Crehore formula run high in value, one-half of all in the table going above those by the first three named formulas. It appears to give better results for hollow than for solid sections. But the large denominator gives small column load so that the formula errs on the safe side.

Results by the new formula are seen in every case but two, to fall between those of the first three "old reliable" formulas, and those two are outside only from one to three per cent. In comparing the means, we see that the new formula falls between the first three. If we reject the Gordon formula, which is now going into disuse in the most approved practice, and compare the new formula

with the 2d and 3d columns of results, we find the mean by the new formula falls between the means of the 2d and 3d columns. Indeed it differs but little over two per cent. from the means of the two columns. This appears the more favorable from the fact that the results in the 2d and 3d columns are obtained from formulas which are probably the best in use to-day in the engineering world.

Comparisons have not been made for other materials than wrought iron, for the reason that wrought iron is the chief material in use at present in large trusses where struts are required.

FORMULAS COMPARED WITH EXPERIMENT.

To make the comparison of the new formula with others in such a way as will be yet more satisfactory to practical men, the breaking load of 33 columns of wrought iron, boxed and open built, such as used in bridges, has been computed and placed opposite the breaking load found by actual test in a testing machine. Also in the table is placed the calculated breaking load as found from the Rankine formula, the Bouscaren-Keystone formula, and of the Rankine formula as worked over by Prof. Baldwin for new coefficients, and given in his article above referred to.

In all cases except in Baldwin's formula, the coefficient in the numerator corresponding to the resistance to crushing, was taken at 40,000 lbs. In Baldwin's formula all the coefficients were used as given in the Magazine. The value of ε for the new formula was used, except in a few cases, as given by the experiments, when given. Corrections were made for pin friction, and for excessive stay spacing for two cases. But these probably effect the final result only 2 or 3 per cent., because the number of corrections needed were so few. But the per cent. for the individual cases was sometimes very considerable. The breaking loads stated are per square inch of cross section of column.

The experimental results cited are from a paper by G. Bouscaren to the Am. Soc. Civ. Engrs. on the strength of wrought iron columns. They are stated by some writers to be from Lovet's report, though I find no mention to this effect in the paper to the society.

The columns cited are of five distinct forms of built columns, viz.:

P. "Phoenix," hollow cylinder, four flanged segments riveted.

K. "Keystone," octagonal tube, four flanged segments riveted diametrically.

A. "American," two flanged bars riveted to the flanges of a central I beam.

S. "Square," hollow, two plates and two channel bars riveted with flanges outward.

O. "Open," two channel bars, flanges outward, latticed with slats, riveted trellis-like.

The signs in the last four columns are proper to make the quantities there appearing, correct the computed values of the resistances to agree with the resistances by experiment.

The sums, + and −, appearing at the foot of the first two columns qualified with signs make it appear that the formulas give results which are too large. Hence it appears that the 40,000 used in the numerator of the Rankine and Bouscaren-Keystone formula is too high. It was used in the Rankine formula for convenience, and because it is found in the Keystone formula. But as Rankine gave the formulas the quantity was 36,000, the same as in the Baldwin formula for flat ends, for which also the denominators agree. Hence the quantities by the Baldwin formula down to No. 5 may be regarded as coinciding with Rankine's. The values in Rankine's space from No. 5 down for round ends should be reduced 10 per cent. to change from 40,000 to 36,000. In order to present the results as would be thus given for Rankine's formulas unmodified, or for the numerator at 36,000, they have been worked out and given in the second set at the foot of Rankine's column of differences. The reduction from 40,000 to 36,000 is evidently too great as in one set the − values predominate, while in the other it is with the + values. An estimate made on this score would indicate that 38,000 would be about the best value for the Rankine formulas, the same indeed as is known to be in use by some Ry. Cos. It appears also that 40,000 is too great for the Keystone formula.

In glancing over the four last columns

TABLE OF COMPUTED RESULTS COMPARED WITH EXPERIMENTS.—BUILT WROUGHT IRON COLUMNS.

No. and style.	Length feet.	Breadth inches.	Sec. area sq. in.	k^2	ε Millions.	Resistance per sq. ins. Experiment lbs.	Rankine formula.	Bouscaren-Keystone formula.	Baldwin formula.	New‡ formula.	Difference, Rankine.	Difference, Keystone	Difference, Baldwin.	Difference, new.
9.K	15	8.85	23.67	7.833	24.7	32000	35900		32300	34500	— 3900.		— 300.	— 2500.
2 "	5	10.1	14.25	11.044	—	33600	39600		35670	39300	— 6000		— 2070	— 5700
25 "	27	12.	18.83	11,424	28.1	21100	31870		28800	30800	— 10770		— 7700	— 9700
24 "	27	9.5	19.20	12 041	26.5	25000	32600		29300	29800	— 7600		— 4300	— 4800
27 "	27	8.62	18.83	9.798	23.7	27800	30800		27700	29800	— 3000		+ 100	— 2000
26 "	27	9.38	14.49	11.178	27.5	27500	31900		28700	29200	— 4400		— 1200	— 1700
30 "	27	9.5	15.13	11.464	19.3	30000	32100		28900	29400	— 2100		+ 1100	+ 600
31 "	27	12.	15.13	11.464	23.6	25400	32000		28800	30900	— 6600		— 3400	— 5500
19.A	27	10.	20.10	13.510	25.0	27800	32900		23900	31800	— 5100		— 1100	— 4000.
10.P	27	8.12	13.70	8.935	19.1	31000	30200		27100	30000	+ 800		+ 3900	+ 1000
29 "	28	8.25	13.58	8.935	28.5	36600	29600		26600	39300	+ 7000		+10000	+ 7300
6 "	15	8.05	14.09	8.536	27.4	37500	36200		32600	35400	+ 1300		+ 4900	+ 2100
28 "	28	8.25	13.58	8.935	25.7	34800	29600		26600	28400	+ 5200		+ 8200	+ 6400
23 S	24	10.	13.70	11.628	28.9	33200	33400		30100	32700	— 200.		+ 3100	— 500
22 "	26	10	13.60	9.347	27.8	30000	31000		27920	30000	— 1000.		+ 2080	± 600
32 "	27	10.5	26.05	10.909	30.1	30200	31600		28400	30500	— 1400		+ 1800	— 300
20.A*	10	8	9.66	10.90	27.6	20000	37400		40600	18600	— 17400		—20600	+ 1400
34.O.*	12.25	9	6.00	18.06	(24.)	17600	38700		34800	18100	— 21100		—17200	— 500
37 "	27.48	14.	12.08	19.98	"	29600	34750		31300	33200	— 5150		— 1700	— 3600
38 "	23.	14.	13.48	20.69	"	32300	36300		32600	34900	— 4000		— 300	— 2600
39 "†	2.	2.8	6.60	0.70	"	35400	39100		35200	38800	— 3700		+ 200	— 3400
40 "†	1.61	2.8	6.60	0.70	"	35700	39100		35200	38700	— 3400		+ 500	— 3000
41 "	27.5	14.	13.74	20.79	"	32400	34960		31400	33600	— 2560		+ 1000	— 1200
42 "	27.5	13.	11.05	21.26	"	32300	32600		29400	30950	— 300		+ 2900	+ 1350
5.K	27.	11.6	13.12	10.945	29.5	22000	19360	26100	19600	20070	+ 2640	— 4100	+ 2400	+ 1930
16.A	20	8.	12.50	5.497	28.9	26700	18500	25200	14760	24000	+ 8200	+ 1500	+11940	+ 2700
13 "	26	12.	25.05	18.215	30.4	24000	25100	30800	17800	26800	— 1100	— 6800	+ 6200	— 2800.
21.S	25.75	10.	13.60	11.000	31.0	25500	20400	27000	20800	25600	+ 5100	— 1500	+ 4700	— 100.
33.O	28.53	10	5.68	20.07	32.4	31700	23200	30300	33600	27600	+ 8500	+ 1400	— 1900	+ 4100
36 "	34.00	10.25	7.48	8.73	(24.)	23130	12800	19400	12310	19800	+ 10330	+ 3730	+10820	+ 3330
11.P	25.19	8.12	13.89	8.935	27.1	21700	18260	25100	18220	20160	+ 3440	— 3400	+ 3480	+ 1540.
17 A	20	10.	19.90	8.733	23.1	26500	23080	29270	24130	25140	+ 3420	— 3770	+ 2370	+ 1360.
14 "	26	10.	20.72	8.733	26.	22000	17870	24800	17900	21300	+ 4130	— 2800	+ 4100	+ 700.
Sum of + discrepancies											+ 60060.	+ 20930.	+85790.	+35810
Sum of − discrepancies											−110780	−132050	−61770.	−53900
Mean discrepancy, regardless of sign											5177.	4636.	4471.	2718.
Mean discrepancy, regardless of sign, rejecting Nos. 20 and 34.											4010.	3469.	3326.	2661.
Rankine formula sum of+discrepancies for numerator=36000.											+102310.			
Rankine formula sum of−discrepancies for numerator=36000.											− 59870			
Mean discrepancy, regardless of sign											4915.			
Mean discrepancy, regardless of sign, rejecting Nos. 20 and 34.											3769.			

For the blanks in this space, the columns have flat ends; and the values are the same as for the Rankine formula. Where values are entered below, the columns have pin bearings.

For the blanks in this space the values are the same as for the Rankine formula.

* Insufficiently stayed or trussed. Correction (118) applied for the new formula † Simply a single piece of channel bar, about 2.8″ ×12″. ‡ For the new formula the flat ends were regarded as fixed because of the enlargement of ends, strengthening by special riveting, &c.

it is seen that in the first the — signs are chiefly among the results for flat ends, while the + signs predominate at the round ends. This indicates that the Rankine formulas are constitutionally at fault, or perhaps that the 1, $\frac{1}{9}^{6}$, and 4, coefficients to the last term of the denominator for flat, flat and round, and round, ends differ too much. As adopted in the Bouscaren-Keystone formula, viz.: 1, $\frac{3}{2}$ and 2, the discrepancies would probably be less, as indeed the column footings show. With these coefficients it would appear that a better numerator would be about 38,000.

The column of differences due to the Baldwin formula are found to be far more satisfactory than either of the two preceding it. That is to say, the signs are more generally intermixed, the + and — values sum up more nearly equal to each other numerically, and the average discrepancy, without sign is smaller.

If, however, Nos. 20 and 34 were cut out from the table, the + and — sums would more nearly agree in value for the Rankine formula than for any of the others. But the mean error without sign would be higher than for any of the others, as indicated by the figures given. Though the Baldwin formula still excels the Rankine and Keystone formulas in the smallness of mean error, yet the difference in + and — sums is greater.

But in every respect we find the last column of figures, which are due to the new formula, by far the most favorable. That is to say, the individual discrepancies in that column run lower in value, the number of + and — values are more nearly equal; the absolute values of the sums of the + and — quantities are lower and differ less; and the mean error, or discrepancy, is lowest. This, together with the fact that the theoretical values, 1, $\frac{1}{9}^{6}$ and 4 are employed, whereas in other formulas, they seem to differ too much, would indicate that the new formula is more nearly theoretically perfect than any of the others. But, in spite of these advantages, the new formula is, unfortunately, less convenient in application than the others, and the question will doubtless arise in practice, whether or no, for an important structure, to sacrifice the additional hour required for the more trustworthy results by the new formula.

As the present object is formulas for bridges, and as wrought iron is the material chiefly employed, no attempt has been made to compare the new formula with others for other materials than wrought iron. It seems probable, however, that where the coefficient of elasticity, ε, is practically constant nearly to the point of rupture, the formula may be used without hesitation. The point of rupture is here meant to be either for rupture by tension or compression. For such a material as cast iron it may be necessary to be guided partly by empiricism, especially for long columns, where the rupture may take place on the convex side by tension, because, in tension, the ε rapidly falls off in value, as rupture is approached. But in compression, ε is more nearly constant, and hence in short columns the formula will probably apply with greater exactitude.

The great difference in the ultimate resistances to tension and compression in cast iron will also cause an approach to rupture on the convex side, relatively, much sooner than for other materials, so that the rapidly diminishing value of ε will effect much shorter columns than otherwise. For cast iron in particular, therefore, it may be necessary to use a low value of the modulus of compression and of ε, and more so as the length increases, say 60,000 to 80,000 for the former, and 12 to 20 million for the latter.

A rational formula must certainly prove much more satisfactory for a comparatively new material, such as some of the grades of steel, than an entirely empirical formula which has not been adapted, that is, in cases where reliable values of the compressive resistance and of ε are known, and not of the constants in the empirical formulas.

MULTIPLYING EMPIRICAL FORMULAS.

Where numerous results of experiments exist for particular classes of columns, it may be possible to adopt an empirical formula for each class, or part of a class, which shall give the results for such columns, or at least to reproduce the particular values to which those formulas are adapted, with closer agreement than any single or perfectly general formula would do. But this process leads to some degree of confusion in the considerable number of formulas produced, and new ex-

periments may develop need of additional formulas, or require a modification of former ones.

In the above tables, comparisons were made only with formulas which are proposed to be perfectly general. But *Trautwine's Pocketbook* cites six different formulas by D. J. Whittemore, C. E., which, accordingly, would be required in computing strains for three of the forms of columns in the above table, and including only 17 of the experimental tests. Though Whittemore gives 12 different formulas, only 6 apply, and half of the experimental columns cited in the table are left by him without a formula.

In a recent paper by Prof. Wm. H. Burr, ten different formulas are produced from twenty-nine experiments. The experiments are mostly cited in the above table, and the formulas, when the whole ten are used, each for its intended form of column, give results agreeing remarkably well with the experiments. The mean error, regardless of sign, is 1720—a quantity less by about 33 per cent. than the mean by the new formula from all the experimental columns cited in the above table, viz., 2661 (two being left out). But when we observe that in this comparison, the new formula, obtained independently of all experiment or specified material, is arraigned before 10 formulas which were obtained from the very experiments compared with, and at the rate of one formula to three experiments, it does not seem strange that the ten formulas should reduce the mean error. Considering the idiosyncracies of column tests, are we at all confident that n extending the application of the 10 formulas to 30 new tests, the mean error will remain as low as indeed either of the values cited?

FACTOR OF SAFETY IN COLUMNS.

The usual way of providing for safe loads for columns is to take a certain fractional part of the breaking load; say such a fractional part as is taken in eye bars. The fraction $\frac{1}{6}$ is common. Thus, the ultimate resistance of wrought iron to tension is about 50,000 lbs. per square inch, and the $\frac{1}{6}$ part, or 10,000 lbs., is almost an absolute conventionalism among bridge builders, and may be regarded nearly in the sense of an absolute modulus of working resistance to tension.

Accordingly, $\frac{1}{6}$ is prefixed to column formulas (Rankine's and Gordon's) by many bridge engineers, as giving the proper fractional part of the ultimate resistance, which is to serve as the safe working resistance. But in empirical formulas, where the composition of the constants is unknown, this is a blind practice, because, from analogy with eye-bar strains, $\frac{1}{6}$ of the ultimate compressive resistance of the *material* is evidently meant, instead of $\frac{1}{6}$ of the resistance of the *column*.

This matter can be corrected in the Rational formulas, and it will be seen that the "$\frac{1}{6}$" coefficient to the empirical formulas for columns gives an unintentionally small safe load. A glance at formulas (114) and (120) will suffice for this. Thus, (114) shows that by using $\frac{1}{6}$ of the ultimate resistance to compression, t, we effect both the numerator and the second term of the denominator, whereas, in (120), we only modify the numerator by taking $\frac{1}{6}$ of the breaking load by formula.

To indicate how the "safe load" is effected by the empirical as compared with the rational method, the following values have been computed for certain ones of the columns in the principal table above.

	No.	l	$\frac{l}{k}$	For $\frac{1}{6}$ Column breaking load.	For $\frac{1}{6}$ t in (114)
Flat end	6	15	5.2	7080	7790
"	23	24	7.1	6540	7680
"	29	28	9.3	5860	7370
Rounded	16	20	8.5	4800	6320
"	36	34	12.	3960	5325

Here the last column of figures is to be regarded as truly representing the resistance of the columns, per square inch of section, when the maximum strain on the material of the column is only the fifth part of the breaking strain, while the column of figures preceding the last, usually regarded as the fifth part of the breaking load, does not strain the material to more than about one-seventh of its ultimate resistance. But fortunately this difference is on the safe side, so that the majority of existing columns in bridges are amply strong.

The figures in the last column are all less than 8000 lbs., the maximum strain assumed as allowable, that is, the fifth part of 40,000 lbs. The differences are to be considered as provisional, for preventing the strains rising in excess

of the 8000 lbs. by buckling or otherwise.

By taking $\frac{1}{4}t$ in place of $\frac{1}{5}t$ for computing the last column of figures, perhaps as close an agreement is obtained with the column preceding it as is possible with a simple fraction. From this fact it appears that the "factor of safety" usually employed for columns has been in the neighborhood of $\frac{1}{4}$, or that the working resistance of iron in columns has been from 5000 to 6000 lbs. per square inch, compression. But as a high value of the working resistance to compression is regarded by some prominent engineers as safer than a like high value for tension, it appears that columns have habitually been made unduly heavy.

SEMI-COLUMNS, WITH PIN ENDS AND COMPRESSION.

Perhaps this term will serve to distinguish pieces which are subject to longitudinal and transverse strains; as in Fig. 2, or in Fig. 28 and following.

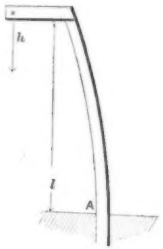

Figure 28.

An expression for the max. strain in the section at A, Fig. 28, is obtained by combining Eq. (8) with (25) and eliminating M. Whence

$$t=\frac{\mathrm{T}hd_1}{\mathrm{I}\cos nl}\pm\frac{\mathrm{T}}{\mathrm{K}} \quad . \quad . \quad . \quad . \quad (124).$$

$$\left.\begin{aligned} &\text{where } n^2=\frac{\mathrm{T}}{\varepsilon\mathrm{I}}=\frac{t'\mathrm{K}}{\varepsilon\mathrm{I}}=\frac{t'_1}{\varepsilon k^2}\\ &\text{also } \frac{\mathrm{T}}{\mathrm{K}}=t', \text{and } \frac{\mathrm{T}}{\mathrm{I}}=\frac{t'}{k^2}\end{aligned}\right\} \quad . \quad . \quad (124a).$$

These quantities may, indeed, be introduced with probable greater convenience for application, giving

$$\frac{t}{t'}=\frac{hd_1}{k^2\cos\frac{sl}{k}\sqrt{\frac{t'}{\varepsilon}}}\pm 1 \quad . \quad . \quad (125).$$

the + sign being used to obtain the max. compressive strain, and the − sign for the max. tensile strain. The piece will fail from the first or second, as depending on the nearest approach of that strain to the modulus of crushing or of rupture. For wrought iron the + sign should be used, while for cast iron, except when h and l are very short, the − sign. The max. strain per square inch is t, tension or compression as just indicated, while t' is the actual average compressive force which is applied to the semi-column per square inch of the cross section. For instance, if the section be 10 square inches and the compressive force applied be 100,000 lbs., $t'=$ 10,000 lbs. But the max. t resulting strain in the semi-column may be $t=30{,}000$ lbs. per square inch. Also h is the distance from the center of gravity of the end cross section to where the compressive load is applied; d_1 the distance from the center of gravity of the middle section to the side, concave or convex, where the strain t is reckoned; k the radius of gyration of cross section; l the length of semi column, and ε the coefficient of elasticity.

When d_1 is taken on the convex side, and the 1st term 2d member $= 1$, we will have tension $t=o$.

Though a direct determination of t is effected for an assumed value of t', yet the converse will doubtless most frequently be desired, but cannot be obtained by a direct simple solution. A few trial values of t' however will give a sufficiently close value for t.

For a case like Fig. 29 it is only necessary to put $\frac{l}{2}$ instead of l in (125); whence, for AB$=l$, AC$=h$, and for pin bearings at C and D,

Figure 29.

$$\frac{t}{t'}=\frac{hd_1}{k^2\cos\frac{l}{2k}\sqrt{\frac{t'}{\varepsilon}}}\pm 1 \; . \; . \; (126).$$

If there be a load, P, at the middle of the semi-column, acting transversely and downward in Fig. 29, a suitable expression

is obtained by substituting M from (23) in (8), observing that for (23), l and P should be changed to $\frac{l}{2}$ and $\frac{P}{2}$ and that the essential sign of M is $-$.

Hence, for pin bearings, and a load P at the middle

$$t=\frac{d_1}{I}\left(\frac{P}{2n}\tan n\frac{l}{2}+\frac{Th}{\cos n\frac{l}{2}}\right)\pm\frac{T}{K} \quad . . \quad (127).$$

in which I = the principal moment of inertia of the cross section K, T the whole compressive force applied, and n as given above. P is $-$ for acting upward.

In order that the flexural moment at the middle of the semi-column be zero, the parenthesis should be zero. Whence

$$h=-\frac{P}{2nT}\sin n\frac{l}{2} \quad . \quad . \quad . \quad (128).$$

This value of h is expected to be negative, and it implies that when P acts downward in Fig. 29, the arms h=BD= AC must be laid off downward.

This equation is applicable to upper chords of deck bridges which carry a floorbeam at the middle of each panel, and for determining the position of the pin bearing such that the max. load shall only cause compressive strain in the middle of the panel length. The chord should however be considerably heavier than as thus determined.

By making $h=o$ in (127), we obtain the expression for a column with pin bearings against the middle of which there exists the lateral thrust P, viz.:

$$=\frac{Pd_1}{2nI}\tan g\, d\frac{l}{2}\pm\frac{T}{K} \quad . \quad . \quad . \quad (129).$$

In deck bridges where numerous floor beams rest on each panel length of the upper chord, as is sometimes the case with wooden floor beams on iron trusses, we have each panel length serving like a column under cross loading, the latter being nearly uniformly distributed.

An expression for such a case is obtained from (35) and (8). Hence for pin bearings

$$t=\frac{wd_1}{n^3I}\tan g\, n\frac{l}{4}\tan g\, n\frac{l}{2}\pm\frac{T}{K} \quad . \quad . \quad (130).$$

$$=\frac{wd_1}{n^3I}\left(\sec n\frac{l}{2}-1\right)\pm\frac{T}{K}$$

where w is the loading per unit length.

With Fixed Ends and Compression.—Where the semi-column is fixed at the ends, as in continuous upper chords, combine (77) with (8) for a load P at the middle; whence

$$t=\frac{Pd_1}{2nI}\tan g\, n\frac{l}{4}\pm\frac{T}{K} \quad . \quad . \quad . \quad (131).$$

which gives the strain at either the middle or end of the panel length.

For a uniformly distributed load, w, for unit length, and for continuous upper chords, we have by (78) and (8)

$$t=\frac{wd_1}{n^3I}\left(1-n\frac{l}{2}\cot n\frac{l}{2}\right)\pm\frac{T}{K} \quad . \quad . \quad . \quad (132).$$

for the end of the panel length, and where the strain t is greatest.

At the middle we will have from $x=\frac{l}{2}$ in (75) and (8)

$$t=\frac{wd_1}{n^2I}\left\{\frac{n\frac{l}{2}}{\sin n\frac{l}{2}}-1\right\}\pm\frac{T}{K} \quad . \quad . \quad . \quad (133).$$

These formulas for distributed loads apply to account for the strain due to the weight of the parts, themselves, as in upper chords of through bridges; and the inclined end posts. In the latter however gravity acts with a transverse component only, which is considerably less than where the piece is horizontal.

Other cases, such as for one pin bearing and one fixed end, is provided for in (63), (64), (65) and (8), two floor beams and pin bearings in (49), &c., &c.

Pin Bearings and Tension.—In lower

Figure 30.

where the longitudinal force is tension, chords of bridges and other cases

we may combine (20) with (8), and obtain an expression adapted to Fig. 30. viz.:

$$t=\frac{d_1}{I}\cdot\frac{2Th}{e^{nl}+e^{-nl}}\pm\frac{T}{K} \quad . \quad . \quad . \quad . \quad (134),$$

where l=AB, and h=BC. Here d_1 will usually be measured on the convex side of beam, and where the + sign will generally be required. Also t will probably always be tension.

The formula for Fig. 31, will be obtained by replacing l in (134) by $\frac{l}{2}$, giving for pin bearings at C and D,

$$t=\frac{d_1}{I}\cdot\frac{2Th}{e^{n\frac{l}{2}}+e^{-n\frac{l}{2}}}\pm\frac{T}{K} \quad . \quad . \quad . \quad . \quad (135).$$

in which l=AB, and h=AD.=BC.

Figure 31.

When a load is applied at the middle, acting downward, we have, by aid of (19) and (8), observing that P in (19) is the half of P here,

$$t=\frac{Pd_1}{2nI}\frac{\left(e^{n\frac{l}{2}}-e^{-n\frac{l}{2}}\right)-2Th}{e^{n\frac{l}{2}}+e^{-n\frac{l}{2}}}\pm\frac{T}{K} \quad . \quad . \quad (136).$$

In order that the flexural moment at the middle of the beam shall be zero, the 1st term, 2d member, should be zero, which condition determines h.

Also h may be zero for a link or eye bar.

For a uniformly distributed load of w per unit length, we have for pin bearings

$$t=\frac{wd_1}{n^2I}\left\{1-\frac{2}{e^{n\frac{l}{2}}+e^{-n\frac{l}{2}}}\right\}\pm\frac{T}{K} \quad . \quad . \quad (137.$$

Fixed Ends and Tension.—For fixed ends we may combine (72) and (8) for a load at the middle, and for a distributed load (73); and these give strains at the end of the panel length. For the middle employ (74).

Other cases may readily be provided for. For instance, for two floor beams to the panel length, the same resting on the lower chord, use (43) for ends in pin bearings.

For one pin bearing and one fixed end we have (68) and (69), &c., &c. The value of M found from any of these equations may be introduced in eq. (8) to the end of obtaining the maximum strain t, either tension or compression. Equivalents expressed in (124a) are applicable in all.

Examples of Strains in Semi-Columns.—The following examples have been worked to indicate the use of the formulas.

Take a wrought iron end post, or a panel length of an upper chord, length 20 feet, constructed as shown in Fig. 32,

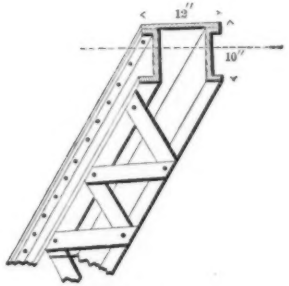

Figure 32.

of two 10″ channel bars of $\frac{1}{2}''\times2\frac{1}{2}''$ flanges $\times\frac{3}{8}''$ web; a plate $12''\times\frac{3}{8}''$, and slats, all riveted together as shown. At the ends, proper thickening pieces, gussets, &c., are supposed in use.

Results for the section, with moment of inertia as dotted, are exclusive of latticing,

K=section=12.875 sq. in.
I=205, axis 3″ from plate.
k^2=15.93 in.=radius of gyration.
d_1=3.36 in. plate side.
" =7. " opposite side.
w=wt. per inch length 3.6 lbs.

As a vertical column with knife bearings or pins without friction at center of gravity of sec. and with a max. strain of 10,000 lbs. per square inch, the resistance by new formula = 9011 lbs. for each inch of section.

As a horizontal column with latticing downward, knife bearings, &c., its weight, causes an additional strain of 459 lbs. per

square inch, as per first member of eq. (130). This is to be added to $\frac{T}{K}$ = either the 10,000 or 9,011 lbs., according to whether the former or latter is to be regarded as the strain per square inch without flexure. The difference between these figures is the provisional excess mentioned under "factors of safety," and is peculiar to columns. This excess should grow rapidly less as we change from partially indifferent column like strains to positive moments, such as provided for in eqs. (124) and (126).

The strain computed from the ordinary formula for a beam supported at its ends, its weight being the load, and T= *o*, is 404. lbs., a quantity less than the above, as it should be.

We observe that the strain due to the uniform load *w* is, by eq. (130) directly proportional to *w* when T is constant, so that if this load be increased to 40 lbs. per inch or 480 lbs. per ft., the strain will be increased to 5,559 lbs. per sq. in., while by the ordinary formula, ignoring T, it is 4,894 lbs. As an end post inclined at 60° with the horizon the strains will be reduced by a half.

Next, let the loading be P at the middle, of 1,000 lbs., with T and other data as before. As a column in pin bearings, Eq. (127), with $h=o$, gives for the strain due to P 1,090. lbs. per sq. in. By the ordinary formula it is 985.

Transferring the pins to the top of end sections, other conditions as in the last, then $h=3.36$ in. in (127), and the strain due to 1st term of 2d member of (127) is $1090+578=1668$.

To find where to place the pins so that the moment of strain and the middle shall be zero, we have from (128) $h=-6.34$ inches, which is within $\frac{2}{3}$ of an inch of the bottom.

As a last example, let the aggregate section of an iron lower chord be 4″ breadth by 5 inches depth. Let the ends be fixed, as when continuous and clamped at the panel points. Let the panel length carry two floor beams, each with center 18″ from panel point. Suppose the severest strain be due to the drive wheels, and when one pair rests directly on one of the floor beams. Then the other wheels will probably not rest on the other floor beam for unequal spacing. To simplify the work, find the effective load on the floor beam directly under the pair of drive wheels, and let it amount to 23,560 lbs.=Q. Also take the point of contrary flexure near the other floor beam as a point of support. This places the chord in the condition of a beam fixed at one end, supported at the other, and having a load applied at 18″ from the fixed end. Take the beam thus conditioned at $l=$ 129.6″ and let T = 88,000 lbs., $\varepsilon =$ 24,000,000. Then,

$$\frac{T}{\varepsilon I} = .000088 = n^2 \qquad n = .009381$$

$$nl = 1.2158 \qquad na = 0.16886$$

$$n(l-a) = 1.0469$$

$$e^{nl} = 3.3729 \qquad e^{-nl} = .2965$$

$$e^{na} = 1.1839 \qquad e^{-na} = .8446$$

$$e^{n(l-a)} = 2.8489 \qquad e^{-n(l-a)} = .3510$$

Then by (69)

$$\frac{P}{Q} = .02968$$

The maximum strain is at the fixed end, where x and $y=o$. Hence, the first member of 68, by aid of P from above, gives

$$M = Qa - Pl = 14.153\ Q = 333500.$$

$$\text{also } \frac{T}{K} = \frac{88000}{20} = 4400,$$

These in eq. 8 give the max. strain per square inch desired,

$$t = \frac{2.5}{41.67} 333500 + 4400 = 24405. \text{ lbs.}$$

The ordinary formulas for the same case and data, computing the parts separately and adding give

$$t = 24800. \text{ lbs.}$$

ON A CASE OF PERMANENT POLARITY IN STEEL OPPOSED TO THAT OF THE MAGNETIZING COIL.—By A. Righi.—As a result of his experiments, based on theoretical considerations, the author finds that, "If we take cylindric bars of steel of the same diameter, we always find, for the same value of the magnetizing force of the coil, a bar of such length that it cannot acquire any trace of permanent magnetism; so that with shorter bars we may obtain a permanent polarity contrary to that which is developed during the passage of the current in the coil."—*Il Nuovo Cimento.*

ON THE MECHANICAL PRODUCTION OF ELECTRIC CURRENTS.

From "Engineering."

II.

We have explained how by expending mechanical power in moving a coil of wire past the pole of a magnet, or by moving the pole of a magnet past a coil of wire, we can produce momentary currents of electricity in the coil. We have insisted upon explaining this action by observing the lines of force in the surrounding space, and laid down the rule that the strength of the current so induced in the circuit was proportional to the rate of decrease in the number of lines of force that cross through the circuit.

It will have been observed that in all these actions it is the *relative motion* of magnet and coil that is concerned. It matters not whether the magnet move towards the coil or the coil towards the magnet, the result is the same, an increase in the lines of force threading through the coils, a decrease in the potential energy of the system, and the production of a momentary inverse current. In the actual construction, however, of almost every kind of dynamo-electric machine, the magnets are fixed while the coils move. This is merely for the sufficient reason that the coils are usually lighter than the magnets, and therefore it would be absurd from an engineering point of view to make the moving parts heavier than the fixed parts. In the generators of Pacinotti, Gramme, Wilde, Siemens, Brush, and Edison, the magnets are fixed while the coils rotate. In Lontin's dividing machine and in a few other generators, however, there is an exception. The magnets attached to a central axis revolve, while the numerous coils in which it is desired to generate currents are fixed upon an external frame.

The next step in explaining how the production of continuous currents in one direction is accomplished, in a typical generator, will be a consideration of the effect of moving a ring of wire up to a magnet and passing it over its poles.

In Fig. 13 is shown a single ring or coil of wire to serve as a circuit, and beyond it a magnet is shown with its north pole facing the ring. From what has been already laid down it is easy to say what will be the result of moving the ring along towards the magnet. While the ring approaches the north pole, as in the first case of the figure, the lines of force

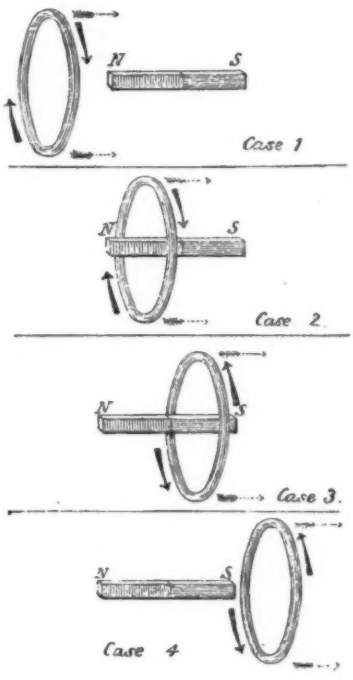

Fig. 13.

that thread through the ring will be increasing in number, and there will be an *inverse* current (*i.e.*, one which is in a direction inverse to that which would cause the pole N to be attracted: compare the figure of case 1 with Fig. 10, and note the direction of the arrows). Moreover, even after the ring has been moved as far as the pole N there will still be an inverse

current so long as the ring by moving forward is increasing the number of lines of force that it embraces. Comparison with Fig. 2 and Fig. 11 will show that there will be an increase in the number of lines of force until the ring has got as far as the center or neutral line of the magnet. The second figure of Fig. 13 represents matters up to that point. But now if the forward motion of the ring be continued towards the pole S and beyond it, the number of lines of force that thread themselves through the ring will diminish, and therefore from the moment that it passes the middle point of the magnet a direct current will be induced in it, as shown by the arrows in cases 3 and 4, and these will continue as long as the ring recedes from S.

Again, suppose we move our ring or coil between the two powerful magnetic poles, as indicated in Fig. 14. We will

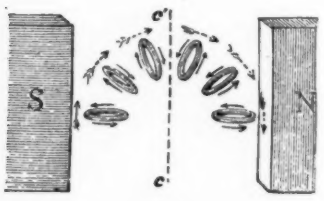

Fig. 14.

suppose that our coil is moving in a circular path between the poles S and N, which are respectively a south and a north pole. The lines of force will run across in almost straight lines from N to S. We will consider what occurs in the coil as it moves round the upper half of the circular path, its first position being on the left. At first it lies horizontal with its plane very nearly along the lines of force, so that almost none of them pass through it. As it rises and turns round the number of lines of force that thread through, the coil increases until it arrives at c', where the number that pass through is a maximum. All this time an inverse current will, therefore, be generated in it. After passing c', and although it is getting nearer N, the number of lines of force that cross through the coil are decreasing, because it is gradually turning round flatways again. From c' onwards there will, therefore, be a direct current induced in the coil. Nor does the direct current cease when the coil arrives at the point nearest N, for now, though not shown in Fig. 14, as the coil goes along the lower semicircle the lines of force will begin again to thread through it, but this time they enter the other side of the coil to that which they formerly entered. There will, therefore, be an increase in the number of lines of force applied negatively, which is, of course, equivalent to a still further decrease. This will go on until the coil arrives at c, where the negative increase is a maximum. All the way round the right hand half of the figure, from c' to c, there will be a direct current induced in the coil. From c onward and round the left hand half of the figure up to c' there will be an inverse current generated. To render such a current available, therefore, as a continuous supply, some device is needed to turn the inverse currents on the left-hand half so that they shall flow into the wires—that go, say, to feed an electric light—in the reverse way to that in which the direct currents flow into them. For if we *invert* that which is already *inverse* we thereby bring it into the *direct* relation. The device for accomplishing this end is called a *commutator*, and the line c c' which marks out the boundary between the currents that are direct and those that are inverse, is known as the "diameter of commutation."

Fig. 15 is a skeleton diagram of the working parts of a Gramme dynamo-electric generator; it is drawn from a model devised by Professor Silvanus Thompson, of Bristol. The well-known ring armature employed in all the machines of Gramme, as well as in the earlier generator of Pacinotti, and the later machines of Schuckert, Brush, and Burgin, is the principal feature. It rotates between the poles S and N of an electro-magnet of a U form with vertical limbs. The commutator consists of a number of separate bars or strips of copper fixed round the periphery of the axis, while above and below are the contact brushes which touch the uppermost and lowermost pieces of the commutator respectively. In the model the ring is enwrapped by a continnous helix of twelve turns; each one of these twelve turns being connected with one of the twelve copper strips of the commutator. In the draw-

ing the armature is supposed to rotate in the same direction as the hands of a clock. The separate turns or coils in the upper half of the ring armature are therefore moving from S towards N, while those of the lower half are moving from N towards S. As in Fig. 14. so here, there will be direct currents generated in all the coils that are on the right hand of the diameter of commutation, *c c'*, and inverse currents in all the coils on the left. The little arrows show the directions of these currents. By following out the various turns of the spiral it will be seen that all the separate currents induced in the individual turns of the spiral on the left of *c c'* are in the same direction along the wire, and therefore all help one another in producing a strong current, though the electromotive force in each may not be equal. A row of horses hitched on to one another in tandem fashion, facing one way, all help to produce one strong pull, though the individual horses may not be equally strong. In these coils the strongest induced electromotive force is, as a matter of fact, in the coils that are passing closest to the poles. In the right-hand half of the spiral the individual currents are all flowing in the other direction. Up each side of the ring armature, therefore, there is a strong current flowing, drawing electricity from the lowest point and urging it towards the topmost point. Now the bars of the commutator, which are here seen end-on in a ring surrounding the axis, are all separate and insulated from one another and from the axis. If electricity were to flow into one of them it would have to flow back again, for there is nowhere for it to flow to; the bars of the commutator lead nowhere. This is, however, not the case for the two bars of the commutator that occupy the highest and lowest points. They touch against the contact brushes which communicate with wires of the external circuit. So the electricity that flows up both right and left of the ring armature can flow into the upper contact brush and thence into the leading wire of the circuit, which therefore has a constant current urged through it. A corresponding action goes on at the lower contact brush; from this point electricity is drawn both to the right and to the left, being supplied by the return wire of the circuit. The current thus furnished is practically continuous; for though the contact is made first against one bar of the commutator and then against another as fast as they arrive at the line *c c'*, it is clear that the currents on the two sides of the ring armature will always be flowing towards that point at which the contact is made. Neither is there any breach in the continuity of the current generated, because one bar of the commutator does not cease to touch the brushes until the next has come up to contact, and because there always are a certain number of turns of the spiral both on the right and on the left in which the elementary currents are being induced. In the actual dynamo-electric generators the coils of the armatures are of course made up of many turns of wire. In the common Gramme ring, for example, there are hundreds or thousands of turns, and these are grouped in twenty-four sections, the wire at the end of each section being joined to the wire at the beginning of the next, and the junction connected directly with one of the twenty-four bars of the commutator.*

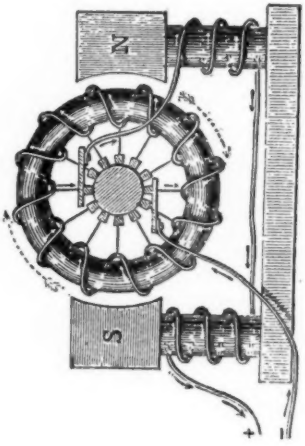

Fig. 15.

Up to this point nothing has been said specifically about the best way of making the magnet poles S and N as powerful as

* The reader, of course, knows that there are other armatures besides the ring armatures of the Pacinotti, Gramme, and Brush machines; there are, for example, the drum armatures of the Siemens machines of modern type, and of Edison's generators. The fraction can be argued out in the same manner, however.

possible. All our later arguments simply treated N as the north pole and S as the south pole of a strong magnet. And in the earliest machines for producing currents thus by the mechanical movement of coils in the presence of magnets, the magnets used to produce the requisite "field" of force were ordinary magnets of hard steel. Hence the name of "magneto-electric machines" assigned to the older instruments invented by Pixii, Saxton, Holmes, Nollet, and Siemens. Hence also arose the name "magneto-electricity" for the currents produced by induction in these machines; for it was at first thought by some that the electric currents that were generated in these older machines, and which consisted of rapidly alternating currents, were due to some kind of electricity other than that which flowed smoothly and continuously out of a voltaic battery. But we know now that electricity, whether the source be frictional, voltaic, magnetic, or thermal, is all the same. The term magneto-electricity is therefore happily dropping out of use. A little later, machines were made in which permanent steel magnets were replaced by electro-magnets. Generators on this principle were constructed by Wilde and by Ladd. A small magneto-electric machine with steel magnets furnished a current which was used to excite the field magnet of a large machine. Then followed the famous quadruple independent discovery of Hjorth, Varley, Siemens, and Wheatstone, that there was no need of a separate exciting machine, but that the generator might serve as its own exciter. Suppose the magnet whose poles are S N, to be *very weak*, the currents induced in the coils of the rotating armature will also be very weak. But lead them round the magnet in a spiral, and they will exalt its magnetism, and if it become more powerful it will react on the coils and induce more powerful currents. So, without any permanent steel magnet to begin with, with only masses of iron rendered (as all masses of iron on the earth are) feebly magnetic by the action of the magnetism of the earth, this "action and reaction" principle serves, when the armature is set rotating, to raise the field magnets in a few instants to the highest degree of magnetization which they are capable of acquiring. The model (Fig. 15) shows this, for the wire attached to the upper brush into which the currents generated in the armature are continually flowing is wound round the upright limb of the soft iron field magnet on the right in a spiral, and then crosses to the left, and forms another spiral round the other limb before it passes out to convey the current into the leading wire of the circuit. Such a machine differs only from a magneto electric machine in the particular, that it uses its own current to magnetize its own magnets; and for the sake of distinction machines of this kind have been called *dynamo-electric machines*. But the fact is that all generators, whether they thus supply themselves with magnetism or not, are as truly magneto-electric in their action as if they worked with magnets of permanent steel; for, as we have seen, there must be in all of them a *field of magnetic lines of force* in which the coils move. All the generators are in this sense "magneto-electric." Moreover, in all these generators the energy of the currents is supplied by the mechanical energy expended in producing rotation of the armature. It is not the magnets that supply the constant flow of power. If of steel they are just as powerful at the end as at the beginning. If they are of iron they are no weaker at the beginning than at the end. It is harder work to push the coils across the magnetic field when there is the current flowing in them than would be the case if there were no current, and this extra work the steam engine has to do. The work so done is not, however, lost, as is the work done in overcoming friction; it reappears as the energy of the currents of electricity thereby generated. The *mechanical power* of the steam engine is the real source of the electric power, and therefore all the generators, whether their magnets be soft iron or hard steel, are in this sense "dynamo electric."

In concluding this section let us recapitulate the various points in our argument concerning the mechanical production of electric currents.

Firstly. Magnetic forces must be studied as they exist in the magnetic field or space surrounding the magnet.

Secondly. A wire that carries a current also possesses a magnetic field in the space surrounding it, and the stronger a current is the stronger will be its field, and the more numerous the lines of force that thread through the circle.

Thirdly. If you alter the strength of the field of a current that flows in a wire by moving a magnet pole in that field, you alter the strength of the current in the wire.

Fourthly. If there be no current in the wire to begin with, and you move a magnet near the wire (thereby altering the number of lines of force in the field round the wire), the motion of the magnet will evoke a current in the wire.

Fifthly. Increasing the number of magnetic lines of force that cross a circuit induces in that circuit a transient inverse current; while decreasing the number of magnetic lines of force that cross the circuit induces in that circuit a transient direct current.

Sixthly. When coils of wire rotate across the lines of force of a magnetic field the currents induced in the coils during half their journey are direct; during the other half inverse.

Seventhly. By using a suitable commutator all the currents, direct and inverse, can be turned into the same direction in the wire that goes to supply the currents of the external circuit.

Eighthly. The current of the generator may be utilized to magnetize its own magnets by being made to flow round them in spiral coils, thereby magnetizing them far more powerfully than any permanent steel magnet can be magnetized.

Ninthly. In every case the energy of the electric currents generated is supplied by the mechanical power employed to drive the generator.

To sum up the matter still more briefly we will add a few words. An electric current flowing in a circuit of wire may be regarded as a magnetic whirl in the space surrounding the wire. If then by moving the coil of wire past a magnet we set up magnetic whirls in the space surrounding the coil we set up electric currents in the wires themselves. Dynamo-electric generators are machines for moving coils of wires past the poles of magnets, there being special arrangements, *firstly*, to procure the setting up of very powerful magnet whirls around the coils of wire, and therefore of very strong electric currents in the wires themselves; and, *secondly*, to turn all these currents into one direction so as to flow in one steady stream through the circuit.

A general theory of the action of dynamo-electric generators for the production of electric currents by mechanical power has been developed in the preceding pages. In these articles we have endeavored to fulfill our promise to explain "how electric currents can be produced mechanically, and how magnetism comes in the process." It remains for us to redeem our further promise to explain the relation of electric currents to the work they can do, and to the energy expended in their production.

We laid down as a fundamental principle that to do work of any kind, whether mechanical or electrical, requires the expenditure of an equivalent amount of energy. And that just as a steam engine cannot work without using fuel, or a laborer without food, so an electric current cannot go on flowing, nor an electric light keep on shedding forth its beams without a supply of energy from somewhere or other.

Now, although, as already explained, we use magnets in order to generate currents of electricity in rotating coils of wire, a magnet is not in itself a source of power. It will do no work for us until we have done an equal amount of work on it. We must pull its keeper away from it before it can pull the keeper back and do work. It is just the same with other forces. An iron weight is not in itself a source of power. It will do no work for us—it will not even drive a clock—until we do some work on it. We must lift it (and spend our energy in the process) from the ground, working our muscles to overcome the pull of gravity before it can do the work of driving the wheels of the clock in its descent. In generating electric currents from magnets in the manner explained in the preceding articles, we, or our steam engines, have to supply the necessary energy. We spend this energy in moving something in opposition to a resisting force. This something happens to be a coil of wire, or a combination of such coils. The force happens in this case to be a magnetic force, and the result of the motion happens to be (by the particular arrangements of the coils and magnets) the setting up of magnetic whirls round the wire, or what we otherwise call an electric current in the wire. But it is we (or our steam engines) that do the work. Take a parallel case in mechanics. House

bells are often arranged so that when you pull the bell against a resisting spring which when you leave go flies back and rings the bell. Now here the spring possesses a certain power called elasticity, and were it not for this it would not fly back and ring the bell. But the elasticity does not of itself do anything. The spring is not in itself a source of power. It is only when we do work in overcoming its resisting force that it reacts or does work for us; and all the time it is we who supply the energy.

Again, an electric current while it lasts represents a certain amount of active energy. A heavy railway truck shunted along a railway line by the push of a powerful engine represents, while it moves, a certain amount of active energy, for if it crashed against a wall it might knock it down. But presently friction brings it to rest. The electric current also comes rapidly to rest if left to itself, being stopped in the fraction of a second by the resistance it meets with in the metal wire through which it flows. To keep the truck speeding on mile after mile requires a continuous expenditure of energy in the generator.

This brings us back to the whole question of the mechanical production of electric currents in the dynamo-electric machine, and to the point how the mechanical energy of a rotating axis (derived from burning coal in a steam engine)gets converted into the electrical energy of a current flowing invisibly along a wire, which seemingly is quite a different sort of power.

We must again point out that the energy of a current is to be conceived as existing in the space *outside* the wire, quite as much as in the wire itself; that the current itself is always accompanied by magnetic whirls in the space round the wire, and that if we can only set up and maintain magnetic whirls in the space surrounding the wire we set up and maintain currents in the wire itself. Now the way to set up magnetic whirls is to move magnets. And as we cannot set up or maintain magnetic whirls without expending energy, it is clear that we shall have to spend energy in moving the magnets.

Let us go back a little way in our study, far enough to take up again the case of the action of an electric current on a magnet. If we go back to Fig. 10 on page 101 it will be remembered that this diagram illustrated the very important experimental fact that where a magnet and an electric current flowing in a circuit act on one another they tend to move so that the number of lines of force that thread through the circuit, increases to a maximum.

Now the lines of force of a magnet have a definite direction of their own, and the positive direction along them is that in which a free north pole would be urged. A free south pole would be urged, of course, in the opposite direction along the lines of force. The action of our simple voltaic circuit (see Figs. 9 and 10) upon a free north pole would be to suck it in from behind, and to drive it right through the circuit, as the arrows show, along the lines of force to an infinite distance in front. The action upon a free south pole would be just the reverse; it would be sucked in from the front and driven out behind. If we began with a single free north pole at an indefinitely great distance behind the circuit it would be gradually drawn up, and more and more of its radiating lines of force would be drawn into the embrace of the circuit. Imagine yourself looking towards the circuit from the point of space occupied by this ideal magnet pole; as the magnet pole is drawn up nearer and nearer you see the circuit subtending an ever-increasing space in the horizon of your vision, just as a sixpenny piece stuck against a window pane covers up more and more of the landscape as you bring your eye nearer and nearer to it. Think of the circuit as the base of a cone having the magnet pole at its apex. As the pole is drawn nearer the apex grows blunter; its solid angle is ever increasing. Now it can be shown mathematically that the *work* done by the circuit in drawing up the pole in this way from an indefinitely great distance is proportional to the solid angle thus subtended by the circuit. This rule is as important in its way as is the rule that the force with which the circuit draws the pole is proportional to the number of lines of force that are drawn into the embrace of the circuit.

We must now apply one of the first

principles of dynamics to the case. *Potential energy always tends to run down to a minimum.* Suppose we have done work by winding up a spring, or by lifting a weight to the top of an inclined plane, or by pulling away from a magnet the iron keeper that was attached to it. In each case the work done is *stored up* as "potential energy": and in each case there is a tendency for the energy so stored to run down. Our spring will uncoil itself; our heavy weight will descend; our magnet will forcibly draw the keeper back; and each of these actions will result in the work of producing motion. To pull our magnet pole back out of the circuit to which it is attracted will require an expenditure of work; it will pull back like a spring; but when we leave go its potential energy will tend to run down and it will be drawn up again into the circuit.

It is clear then that by pushing the magnet pole up towards the circuit we diminish the mutual energy of the system, while pulling the magnet pole back from the circuit or coil of wire we increase the mutual energy of the system.

But now arises the question; in what part of the system is it that the energy is thus increased or diminished? Is it in the magnet pole, or in the circuit, or in the space between them? The magnet pole is certainly not the seat of the change, for its strength remains the same in all the operations. The change of energy must therefore be either in the strength of the current or in the strength of the "field" of magnetic force that surrounds it. And as you cannot change one without changing the other, we conclude that both are affected.

The application of this very simple principle leads us therefore inevitably to the conclusion that when we push the pole up nearer to the circuit (and diminish the potential energy of the system) this very action diminishes—so long as it lasts—the strength of the current in the wire coil that forms the circuit; and that on the other hand when we pull back the magnet pole out of the circuit (and increase the potential energy of the system) we thereby increase for the time the strength of the current in the circuit.

And if the original current flowing round the coil were *nil?* What then? The argument still holds good, but with this important difference. Seeing that the lines of force of the magnet are the only ones originally belonging to the system, the only way in which they can increase is by a current starting in the circuit, and the only way they can diminish is by a counter current starting in the circuit. This is precisely the result which experiment proves to take place. For, as shown in a previous article, when a magnet pole is pushed up into a coil, it produces a momentary inverse current, and when it is drawn away it produces a momentary direct current in the coil.

To produce powerful currents by a dynamo-electric generator it would therefore appear that arrangements must be made for the revolving coils to cut as many lines of force as possible in the shortest possible time. This implies (1) that the armature should rotate very rapidly; (2) that it should be placed in a position where the field of magnetic force is very strong; (3) that it should consist of many turns of wire, each inclosing as much area as possible; and (4) it should offer a very small resistance to the current. These conditions are not all equally attainable at once. We will examine them separately.

(1). *The Armature should rotate very rapidly.*—The strength of the induced current being proportional to the rate of decrease in the number of lines of force that traverse the circuit, the advantage of rapid rotation is obvious. In the case of those machines in which the armature revolves in a magnetic field of constant strength (as in the case where the field magnets are of hard steel), the strength of the current is almost exactly proportional to the number of revolutions per second. But if the armature be very heavy, or the driving machinery unequal to its work, there is a limit to the speed that can be maintained with efficiency. The writer has seen a small Siemens generator driven at a speed of 2500 revolutions per minute, yielding a current of about 21 ampères, while in contrast to this Edison's large dynamo-electric generator exhibited at the Paris Exhibition produced a current of 900 ampères with a speed of only 350 revolutions per minute.

(2). *The Armature should be placed in a very Strong Field of Magnetic*

Force.—It is for this end that the field magnets are constructed to concentrate the lines of force as much as possible across the space where the armature revolves. In the case of the Gramme ring the iron in the armature itself aids in intensifying the field of force. Three years ago the writer of these lines insisted emphatically in the columns of *Engineering* on the necessity of making the field magnets of dynamo-electric generators on a more massive scale. Edison has done this on his large generator mentioned above, in which the field magnets weigh nearly 10 tons, and consist of seven cylindrical coils, 8 feet long, terminating in enormous blocks of soft iron as cheeks.

(3). *The Armature should consist of many Turns of Wire, each inclosing as much Area as possible.*—Each turn of the wire adds to the inductive effect, for if each cuts across the lines of force of the field there will be an electromotive force set up in each. If there are a hundred turns of wire in the coil that is moved, there will be a force urging a current forward a hundred times as great as if the coil consisted of but one turn. It does not follow that the whole current will be a hundred times as great, because the hundred turns will offer a hundred times more resistance to the flow of a current (supposing the wire to be of the same thickness as the "one turn"). Practice only can dictate the choice of the thickness of the wire and the number of turns. Where currents of very great electromotive force are desired a thin wire of many turns is right; but where (as in electro-plating) it is desired to have a great quantity of current with a low electromotive force, the turns are few and a very thick wire is used. Again, if the area enclosed by the turns is large, a great many lines of force can pass through them; but to make room for our large turns the magnet poles must be wide apart, and this makes the "field" weaker, as the lines of force are not so concentrated as before. It is here again a matter for experience to determine. One point may be noticed, however, namely, that if we try to double the power of a coil by doubling the diameter of the turns instead of doubling their number we enclose four times as great an area. In Edison's monster generator mentioned above as furnishing the extraordinary current of 900 amperes, it is not desired to raise the electromotive force much above 100 volts, and the coils are, therefore, comparatively few. And instead of being made of stout copper wire they are constructed of solid copper bars bolted round the periphery of a drum, so as to offer an excessively small resistance to the currents.

(4). *The Coils of the Armature should offer a very small Resistance to the Current.*—The importance of this rule is obvious to any one who is acquainted with Ohm's law. The waste of energy, occasioned by the heating effect of the current on every part of the circuit that offers resistance to the flow, is a sound economic reason for keeping down the useless resistance of the armature.

Lastly, of these considerations respecting the relation of the electric current to the mechanical energy spent in producing it, we would recall the all-important law of Joule, which is that the energy of a current is proportional to the square of its strength. If you can by any means double the strength of a current you will increase fourfold its power to do work. This law resembles the law in mechanical science to the effect that the energy of a moving body (or its *vis viva* as the old books used to say) is proportional to the square of its velocity. An example will illustrate the point. Suppose a cannon ball weighing 1 lb. to be shot out of a gun with a velocity of 100 feet per second. A certain quantity of energy in the form of the explosive activities of the gunpowder must be expended on it to give it that velocity, and by virtue of its velocity and its weight it is capable of doing a certain amount of destructive work. To shoot *two* such cannon balls at the same rate of 100 feet per second will obviously require the expenditure of twice as much gunpowder, and the two balls will do twice as much damage in the end. Now instead of sending *two* cannon balls at the same rate of 100 feet per second, let us send *one* cannon ball at the rate of 200 feet per second. Will the same quantity of gunpowder suffice? Will the effective work be the same? No, indeed. You will find that you want a *fourfold* charge of gunpowder to impart a *double* veloc-

ity; and that by virtue of the double velocity the destructive work will also be increased *fourfold*. Treble the velocity while the weight moved remains the same and you increase its effective energy ninefold; but then you will require a ninefold charge of powder to produce that trebled velocity. The energy of a moving body is proportional to the *square* of the velocity with which it moves.

Now in like manner the energy of the electric current is proportional to the square of the strength of the flow, that is to say is proportional to the square of the rate at which electricity is conveyed through the circuit. And as the heating effect of a current is proportional to its energy it follows that the heat produced by a current is proportional to the square of the strength of the current.

Again, it has been shown again and again, by experiment, that in a generator in which the magnets are either permanent or excited by a separate current, the strength of the current through a circuit of constant resistance is proportional to the speed of the machine, that is to say, to the velocity with which its armature rotates. Now, by the foregoing principle it is certain that the mechanical work done in driving the armature will be proportional to the square of the velocity of the rotation. It is therefore intelligible that the energy of the current should be proportional to the square of the strength of the current if the current be proportional to the speed. For, by the principle of the conservation of energy, the work which the generator will do can never be more than the work done by the engine in driving it, and should be precisely equal to it in amount if there is none lost wastefully in the process.

REPORTS OF ENGINEERING SOCIETIES.

AMERICAN SOCIETY OF CIVIL ENGINEERS. The Society met Wednesday evening, April 19, Maj. Geo. W. Dresser in the chair. Mr. John Bogart, Secretary.

Mr. Ricardo Orozco, C.E., of Mexico, exhibited and explained the Plans and Profiles of the proposed works of drainage of the Valley and City of Mexico. The explanations were translated by Mr. Theophilus Masac, C.E.

The City of Mexico is situated in a basin without natural outlet. The Lake Texcoco within a very short distance of the city, in times of flood, overflows and affects deleteriously the city, to such extent that its sanitary condition has become very bad.

A short distance further from the city are the lakes Chalco and Xochimilco which also overflow towards the city.

Three other lakes at more considerable distances are in the same basin. There are no natural outlets, only evaporation lowering the areas of the waters.

The extreme desirability of securing drainage from this basin has been long felt.

In the 17th Century, Senor Enrico Martinez, an Engineer under the Spanish authorities, constructed a tunnel partially through the Mountain Nochistongo, which, however, never was entirely completed. Many years afterward the Jesuit Fathers made an open cut down to the tunnel. This work cost a very large amount of money and many lives. Proper slopes were not maintained and the earth caved in frequently.

The drainage has never been properly kept up.

Senor Orozco's plan is to construct an open canal upon such a grade as will entirely drain the lakes Xochimilco, Chalco and Xaltocan, and also maintain at regulated surfaces the lakes Texcoco and Zumpango.

Through the City of Mexico are to be constructed sewers flushed by the waters from the lakes which are carried to a common conduit, where the sewage is purified by deposition, the solid matter to be used for fertilization and the water carried away in the canal. The whole length of the canal would be about 50 miles. Expense about $7,000,000.

Maps, Profiles and Plans executed in a remarkably fine manner were exhibited.

May 3, 1882.

The Society met at 8 P. M. President Welch in the chair and John Bogart, Secretary.

A paper on the Improvement of the Potomac at Washington, by Wm. R. Hutton, member of the Society, was read by the Secretary. The different plans proposed were described. Those of the Board of Survey of 1872, of a Committee of Taxpayers in 1877, of Col Abert in 1878, of the Commissioners of the District in 1879, and its modification in 1881, all preserve the main channel of the river on the Virginia side, and leave a large area of flat low land to be reclaimed along the present water front of Washington, virtually extending the city streets about three-fourths of a mile beyond the present wharves. In some of the plans a narrow channel adjacent to the present shore line is retained for a part of the lower city water front. The objections urged against these plans are their excessive cost; the removal of the water front too far from the business portion of the city; the increased difficulty of drainage; the reclamation of too much land of a character very expensive for maintenance, and in the project of the District Commission special difficulties connected with the proposed flushing basins.

The author advocates the deflection of the

main channel of the river below Georgetown, across the flats to the Maryland shore, and its maintenance substantially along the present water line of the city. He claims that this is entirely practicable, that the low land left on the Virginia side will be less objectionable, that the deep channel close to the city will be of permanent value, that the sewage and drainage of the city will be much more easily disposed of; that there will be less danger of ice gorges, and that the cost will be moderate.

ENGINEERS' CLUB OF PHILADELPHIA.—April 1st, 1882.

President Rudolph Hering in the chair.

Mr. D. McN. Stauffer read a paper upon "Brickwork under Water Pressure."—During the construction of the tunnel under Dorchester Bay in Boston, it became necessary to con-truct a brick bulkhead that had to withstand the pressure due a water column, 162 feet high, and, as it was intended to shut off a leak amounting to 240,000 gallons per 24 hours, it had to be water-tight.

The tunnel section at the point selected for the bulkhead was practically 10 feet square. The bulkhead was built directly across the tunnel, 50 feet in front of the heading where the water was struck. Plank dams filled with puddle clay were first thrown across the tunnel, each side of the bulkhead site, and a 6 inch wrt. iron pipe used to carry off the water during construction—the pipe being built into the brickwork. An arched form was adopted, 4 feet thick at the crown and 2 feet rise in a span of 10 feet. Hard, burned bricks were used, laid in mortar made of one part Eng. Portland, one part Newark Rosendale, and two parts clean, sharp sand—a compound found equally as strong as Eng. Portland and sand, one to one, and having the advantage of working smoother on the trowel, and adhering better to brickwork in wet places. Skewbacks for the arch were roughly picked out in the rock at the sides. After the cement had set, the water was shut off by screwing a cap on to the outer end of the 6 inch cap The pressure against the wall was 72 1/5 lbs. per square inch, or about 519 tons distributed over the face of the bulkhead. The wall was tight for about 48 hours; then the water came through the *brick* itself, rather than through the joints, in amount equal to one-half the original volume.

The water was let off and a second experiment tried. The main wall was torn down sufficiently to allow men to pass behind it, and a second wall only 12 inches thick was built back of the first, and 2 feet distant. The space between these walls was well rammed with puddle clay, extending to the rock on all sides. The second wall was made purposely light, as its yielding to the pressure would only more effectually consolidate the clay between it and the unyielding wall in front. The bond used in the main wall was one so laid, that there were no continuous horizontal joints through the wall.

The result of the last construction was to completely shut off the water from this leak, at a great saving of fuel and pumps. The Dorchester Bay tunnel is 7,000 feet long, with its average invert grade 143 feet below low tide mark in the bay under which it passes. The tunnel is driven through a formation of clay, slates, and conglomerates for its entire length; rock, very seamy, and much water encountered.

Mr. E. V. D'Invilliers read a paper by Mr. O. B. Harder, of the Pennsylvania Geological Survey, entitled "Topographical and Geological Modelling," in which the method pursued by the survey was explained, and the text illustrated by photographs and models The requisite for a good model is a good contour map of the ground to be illustrated, with contours, 10, 20 or 100 feet vertically apart. The lumber used by cigar-box makers is best suited for modelling—of a thickness required by the vertical scale. On a scale 1,600 feet to an inch the boards will be 1/16 of an inch thick and represent 100 feet in height. The contour line marked upon the base and the one next above it are traced upon boards cut the exact size of the base, and the lowest or outer one of the two is sawed through by a jig saw, the upper or inner one serving to mark the place for the next succeeding board. These are nailed on with sprigs—glue being used where from the sharpness of a piece—a nail would split the wood. The wooden model is then waxed—the wax being worked down until just the edges of the boards are seen, and until it presents a smooth appearance and fac-simile of the topography. The model is then rubbed with linseed oil and is then ready for casting in plaster of Paris. Care should be taken to get the oil in on the low places, as on this depends the facility with which the negative can be removed after the setting of the plaster. A cast is then made with plaster of Paris, filled in around a frame to the requisite thickness—and, after being allowed to dry for about an hour, it is removed from the original, smoothed up with a file or knife edge and coated with shellac varnish. From this cast a positive is made—pursuing the same method as before—and is dried and scraped. The sides are smoothed up for the sections, and after being varnished the model is ready for the coloring—representing the different geological formations. Streams and geographical features being added, the model is complete.

ENGINEERING NOTES.

THE NEW YORK DOCKS.—By a resolution adopted by the Dock Commissioners, in July last, the following queries were submitted to an invited board of consulting engineers:

1st. Is the wall now being constructed the most desirable one for all the interests of the city, for the section of the water-front under improvement?

2d. If it is not, what changes in its construction should be made to make it so?

The Board consisted of General John Newton, General Q. A. Gillmore, William E. Worthen, Esq., the Engineer-in-Chief of Docks.

George S. Greene, Jr., was requested to af-

ford all facilities in his power for the examination.

The following is an abstract of the Report:

To the Commissioners of the Department of Docks of the City of New York.

GENTLEMEN: Agreeably to the instructions of your letter of September 14, 1881, and your personal explanations in the interview of the 23d of the same month, we have made "a thorough investigation of the manner of constructing the wall now being erected by the Department," and, as pertinent to your inquiries, an examination of the walls already built by the department on the same general plan.

We have seen the manner in which the concrete blocks were made, the class of materials used, the proportions in which the cement, sand and broken stone were mixed, their deposit in the boxes, and the manner in which the manipulation was conducted, and have tested the absorption of salt water by the concrete. [Details of the examination omitted.]

From our experience and late observations on the old work, and from the investigations on the work now in progress, we feel prepared to answer the questions of your letter of September 14.

In answer to the first query, we would say that this wall, taken as a whole, is the result of practical growth from the first inception of the work. It combines, in a suitable measure, the necessary elements of strength, endurance and stability; it is ample in its dimensions, has a liberal factor of safety, is not difficult of construction, and appears thus far to have been well and faithfully built. We do not therefore recommend, as necessary, any essential changes of plan, but there are certain modifications which we do recommend, and which will be found in detail in answer to your second query.

In answer to the second query, we are of the opinion that the concrete used in the blocks, as well as that formed *in situ* as backing, is unnecessarily rich in Portland cement, and that the best varieties of natural cement would answer very well for this concrete.

If natural cement be used, the proportions to be as follows:

Cement, 1.	For the base of the blocks up to the height of one foot above the chain hole.
Sand, 2.	
Broken stone, 5.	
Cement, 1.	For the balance of the concrete block and the backing.
Sand, 3.	
Broken stone, 6.	

If Portland cement be used, the proportions may be:

Cement, 1.	For the base of block to height above indicated.
Sand, 2½.	
Broken stone, 6.	
Cement, 1.	For balance of block and backing.
Sand, 3.	
Broken stone, 7.	

We also recommend the use of a machine for mixing the concrete, whatever kind of cement be used, and that the blocks be kept, before they are put in the wall, at least sixty days, shaded from the direct action of the sun.

The rip-rap in front of the wall covers the head of the piles to about the depth of two feet; whilst in this condition it is ample protection for the piles from the *teredo;* but should the dredging of the slips be carelessly done, and the excavation be carried into the rip-rap, it will settle away from the walls, and the piles will be denuded, and this may obtain from settlement of the rip-rap without dredging, from the action of the screws of steamships or other causes; and if the piles be exposed, they might soon be destroyed by the worm. To obviate this, we recommend that the base of the concrete block be established at a level of 17 feet below mean low water mark, that is, adding 1 foot 9 inches to the depth of the bottom of this block, preserving the same curve of outer face, and the vertical rear; the position, as well as the bulk of the rip-rap, being kept as at present.

The thickness and weight of the revetment wall, increased by the expedient of your engineer, by the inexpensive filling of cobble stones between the piles, and forming a part of the wall, afford considerable resistance to the earth-thrust of the back-filling.

The rip-rap is also indispensable to prevent the heeling of the mass outwards around a lower point, and in this office, the bracing piles also lend their assistance. The weight of the wall is directly supported by the skin resistance of the piles, and by the mass of cobble stones resting upon the mud. Contributing in considerable measure to this, the rip rap extends the bearing surface on each side of the wall, and acts as braces to prevent overturn or settlement.

The whole mass resting thus upon a yielding foundation, and to be regarded, in some sense, as floating upon it, will, it is thought, be subject to settlement, but not considerable, and experience has shown that the whole mass will substantially settle together, with little variation, and the front of the revetment will retain its *batir.*

With walls thus supported on mud, it is always desirable to establish as great a symmetry of mass as the other conditions imposed on the structure will admit, in order that the settlement, if any, should take place without tilting.

In this view, we would recommend that the rip-rap on each side of the wall should be in nearly equal masses. Some excess may be given to the portion in the rear of the wall, as the extra thrust may be met by the inclined piles.

We recommend that the ballasting with small cobble stones, the placing of the concrete block, and rip-rap work, be completed as much in advance of the other work as possible, in order that settlement may take place before the wall is completed.

For back-filling, under these circumstances, any material will answer; sweepings of streets, collections from houses, ashes, garbage beneath the water, and even dredgings, if not in a liquid state, and all done slowly, working from wall toward shore.

We recommend that where no wall has yet been commenced, and where it will not interfere with wharfage, an experiment be made of

filling in broadly on the line of the wall before doing anything else, as by this means the soft mud will be forced out, or in some degree consolidated by its mixture with firmer material.

The present cross-section of the wall, as designed by your Engineer-in-Chief, we regard as founded on correct principles of stability and economy, and the modifications we suggest seems to us to improve it without changing the ideas characteristic of the plan.

Respectfully submitted,

(Signed) JOHN NEWTON.
Q. A. GILLMORE.
WM. E. WORTHEN.

ORDNANCE AND NAVAL.

KRUPP'S MUZZLE PIVOT GUN.—The Germans seem determined to be ahead of this or any other country in their practical efforts towards the adoption of every new idea in scientific warfare that will give them power in Europe. Once more Herr Krupp has come to the front. This enterprising maker of warlike material has recently conducted a series of experiments with a new kind of gun and shell. The gun is on the muzzle-pivoting system, and the shell has been especially designed for torpedo effect, that is, to burst on penetration of armored ships with a result similar to the explosion of a torpedo. The idea of the muzzle-pivot gun is not novel. It has been known to our War Office authorities for some years; but they have not thought proper to thoroughly or practically test its utility. They have during late years been either allowing what little inventive faculty they possess to lie dormant, or have been content with watching the operations of other powers in the direction of improvements in ordnance and other warlike material, and then copying their results. Unfortunately, the latter has only become too patent, and the position which Great Britain has consequently slipped back to is now admitted by every practical or scientific person. Herr Krupp's recent experiments at Meppen were considered to be highly satisfactory, and quite sufficient to justify the great German manufacturer of weapons in taking immediate measures for the production of larger guns and shells than those tried. The gun experimented with was of 21 centimeter caliber, with a long shell having a tremendous bursting charge, so arranged that the shell should explode only after penetrating some distance into the armor plating. The gun's muzzle-pivot is carried down into a socket fixed in the hold of a vessel in such a way as to prevent the slightest recoil even with the heaviest charge. Herr Krupp's gun was worked during the trials with great ease and certainty of aim, and obtained for the shell a very high velocity. This description of weapon has been designed for gunboats built to carry guns up to 40 centimeters. These gunboats are to be of light draught, high rate of speed, and exceedingly handy. In fact, two or even three of such armed boats would be very ugly customers for a first-class armored ship to cope with, owing to their rapid power of maneuvering, and their small size rendering them difficult to hit. Their cost would be but an eighth or a tenth of a first-class ironclad. The Germans are certainly a very practical race. A good idea once conceived and well considered in all its bearings, they then do not take very long to work it out. We shall hear more ere long of Herr Krupp's muzzle-pivoting guns and torpedo shells.

RAILWAY NOTES.

A CORRESPONDENT has sent the London *Times* the following table, showing what may be called the maximum speed accommodation to or from London, provided by nine different railway companies, and the fares per mile at which they respectively provide it. Wherever the length of the line is sufficient the run taken is considerably over 100 miles, with at least one intermediate stoppage:

No	Railway.	Test Station.	Miles from town	Speed. Miles per hour	1st Class Fares. Pence per mile.
1	Great Northern	York	188	48.000	1.755
2	Great Western.	Bristol	118	45.384	2.652
3	London, Chatham and Dover	Dover	78	44.571	3.076
4	Midland.....	Derby	127	44.046	1.606
5	London and N. Western	Crewe	158	43.880	1.835
6	South-Eastern.	Dover	76	42.617	3.157
7	South-Western	Sherbo	18	40.227	2.440
8	London, Brighton & S.-coast.	Brighton	50	40.000	2.950
9	Great Eastern.	Norwich	126	36.000	2.000

It is obvious that the shorter lines—the London and Brighton, the Chatham and Dover, and the South-Eastern—are unduly favored on this principle, and occupy a higher place than they deserve, and this is especially remarkable with respect to the Great Western, which runs distances a little over that chose, at an equal or greater speed than that given for the Great Northern. The fares of the two latter speak for themselves, and it should be remembered that the boat trains—for which credit has been given—start at very inconvenient hours for passengers, and are almost the only good trains which they run. The performance of the Great Northern and the fares charged are astonishing. The Great Eastern now runs some good trains, and the Midland trains are good and the fares low.

IRON AND STEEL NOTES.

MALLEABLE CAST IRON AND THE ANNEALING OF STEEL.—By M. Forquignon.—Since the time of Reamur who, in his classic memoir of 1722, first formulated the principles of the operation, the manufacture of malleable iron has become of great importance, but, in spite of the improvements introduced in its production, the chemical history of this product has still to be written.

Up to the present all that has been known is that malleable iron is cast iron deprived by

oxidation of a part of its carbon, and thus approaching the state of steel or wrought iron.

To fully understand the experiments, attention is drawn to the following points :—

1st. All pig iron is not equally adapted for the process. Grey or manganiferous pig gives bad results. White pig made from the purest ores is preferable.

2nd. The softening of the iron applies nearly exclusively to small objects ; the operation does not succeed well in the case of objects more than an inch in diameter.

3rd. The time for baking varies, according to size and quality required, from twenty-four hours to six days or more.

4th. The temperature of the furnaces is generally bright cherry red.

5th. Peroxyde of iron (powdered red hæmatite) is almost the only substance now used as cement.

The object of the author has been to determine the chemical composition and tensile strength of numerous samples of malleable cast iron differing from one another either in composition, in place of manufacture, or in the treatment they had undergone, the effects of different substances used as cement, and also to find out in what way malleable iron differs from steel, wrought iron, and cast iron In order to attain the objects in view, the author conducted a series of analyses of cast iron before and after its transformation, and experiments as to its physical properties, especially its elasticity.

The first series of experiments were made with cast iron heated in contact with powdered hæmatite. Secondly, heated with various substances, such as charcoal, iron filings, silica, quicklime, calcined bones in powder, colcothar, and marine salt. Thirdly, in contact with various gases, such as hydrogen and nitrogen. These experiments proved that the softening of the cast iron does not result from the oxidation of its carbon, but that this oxidation, when it does take place, is purely accessory. By the simple action of a temperature sufficiently high, but inferior to its melting point, the white cast iron is destroyed or becomes carbonized, so to speak. A veritable amorphous graphite belonging to a particular variety, is deposited abundantly through the whole mass of the bar, sometimes in the form of little visible agglomerations, sometimes disseminated or intimately mixed.

Instead of a combination there is now only a mixture of free carbon and free iron. The cast iron, deprived of a part of its combined carbon, is softened, and its resisting properties are greatly modified.

The action ends here if the heating takes place in an inert substance like charcoal, but if the iron is in contact with a substance capable of burning or absorbing the carbon, a secondary reaction takes place, the free carbon on the surface is burnt, a portion of the graphite from below comes to the surface and in its turn is destroyed. Manganese in the iron prevents softening by opposing the formation of graphite.

The author sums up as follows :—

1st. A cast iron really malleable always contains amorphous graphite.

2nd. A cast iron may lose carbon and still remain brittle if graphite has not been formed, or if the existing quantity of graphite is not increased.

3rd. A cast iron can become malleable without losing a sensible portion of its total carbon.

4th. If silicon is added to a manganiferous cast iron, the latter is improved by the reheating.

5th. Hydrogen and nitrogen combine with the carbon in the cast iron, which in this case alone can become malleable without the production of graphite.

6th. The breaking weight is always more than doubled, sometimes more than quadrupled, by the reheating; it increases with the length of the heating very rapidly at first, very slowly afterwards. Finally, malleable iron occupies a place intermediary between steel and grey pig; it differs from the latter by the special nature of its amorphous graphite, and by its great tenacity, and from the former by its large quantity of graphite.

A series of experiments with steel similar to those with cast iron, viz. reheating in contact with various substances, confirmed the conclusions arrived at above.

The last few pages of this paper contain a description of a small furnace by which very high temperatures can be easily obtained.—*From Abstracts of Institution of Civil Engineers.*

RIVETING.—The direct crushing strength of wrought iron is generally about equal to its tensile strength, in the case of short isolated bars, but the metal around the rivet hole in a boiler plate is in a different condition, being supported by the surrounding unstrained metal of the plate. Direct experiments which have been made on plates, with the object of ascertaining the crushing strength under these circumstances, show on an average the crushing resistance may be taken at twice the shearing resistance, consequently, in order that the shearing resistance of the rivet and the crushing resistance of the plate may be equal, the area of the rivet shank should be equal to twice the product of the thickness of the plate multiplied by the diameter of the rivet. Deducting from this the value of the diameter of the rivet in terms of the thickness of the plate, we find that it should be two and one-half times the thickness of the plates. This is the theoretically correct size of the rivet, but practically it is found necessary to make the rivet somewhat smaller, for the following reasons: First, if the rivet is given the above size, when we reach thicknesses of plate above seven-sixteenths of an inch, the rivet becomes too large to be properly closed by hand riveting; second, the material of the plate is somewhat injured by the punching, and cannot always be relied upon to bear twice the shearing strength; third, it would be necessary to space the rivets so far apart, to retain equal strength of plate and rivet, that it would be impossible to make a steam and water-tight joint; fourth, the plate is more apt to be reduced in thickness by corrosion than the diameter of the rivet is, so it is found better to give it an excess of strength at

the start to allow for wear. For these reasons the diameter of the rivet is made as small as twice the thickness of the plate, and even smaller for the thicker plates. By placing rivets in two rows, which is equal to one-half of the pitch, a much tighter and stauncher joint can be obtained than is possible when they are placed in the form of an equilateral triangle, as they generally are, which necessitates a reduction of the pitch to obtain a tight joint.

BOOK NOTICES

PRACTICAL MICROSCOPY. By George E. Davis, F. R. M. S. London: David Bogue. Price $3.00.

The rapid increase in the use of the microscope, as a means of research, has led to a demand for good manuals of instruction.

This treatise is not so suitable for the beginner as some books already in the market, but it will prove serviceable to the worker who is interested in the relative merits of the various objectives and stage fittings, about which the more experienced microscopist is likely to be interested The instruction in regard to preparing and mounting specimens is particularly good. The illustrations are fairly abundant, and only fairly executed.

PRACTICAL VENTILATION AND WARMING By James Constantine. London: J. & A. Churchill. Price $2.40.

The principal merit of this book lies in the descriptions of the heating and ventilating equipment of several large public buildings, and as those examples have been selected in which the methods have received public approval, they are presumably safe guides.

Too much space is given to the description of heaters and stoves, which are illustrated by some cuts of inferior quality.

The usual essay on the constitution of the atmosphere, and Dr. Angus Smith's conclusions about the normal proportion of carbonic dioxide of course begins the treatise.

ELEMENTS OF WAVE MOTION RELATING TO SOUND AND SIGHT. By Peter S. Michie, Professor of Natural and Experimental Philosophy in the U. S. Military Academy and Brevet Lieut. Colonel U. S. Army. New York: D. Van Nostrand.

This text-book was prepared expressly for the Cadets of the West Point Military Academy. It is therefore adapted to the proficiency of students who have completed the study of Elementary Mathematics, so far as to include the calculus, and have been through a course in Analytical Mechanics.

This treatise is divided into three parts, viz.: Wave Motion, Acoustics, and Optics.

As the author explains in his preface he has been governed in the arrangement by the necessities of the case and the restrictions of the course of study. He has therefore deemed it advisable to deduce Fresnel's Wave Surface as expeditiously as possible, incidentally establishing the essential principles of undulatory motion, common to sound and light.

Acoustics is made subsidiary to Optics by utilizing its numerous illustrations in vibratory motion, so that the laws of this motion may be more clearly apprehended in the subject of light.

The course in Optics includes a discussion of the essential principles of the deviation of light by lenses and mirrors, and the application of these principles to Telescopes, the Spectroscope and the Polariscope.

The book will be widely read by other students than those at West Point, because it is a prescribed text book at the leading Scientific School of the land, and it is more than likely that Prof. Michie's course in Wave Motion will determine the course of study in our higher Technical Schools.

HANDBOOK OF THE POLARISCOPE, Adopted from the German of H. Landolt. By D. C. Robb, B.A., and V. H. Veley, F.C.S. Price $3.75.

This thoroughly scientific treatise is dividde into the following distinct sections:

General Aspects of Optical Activity, Physical Laws of Circular Polarization, Specific Rotatory Power, Process of Determining Specific Rotation, Practical Applications of Rotatory Power, Rotation Constants of Active Substances.

As might be supposed the practical applications relate largely to Sugar Analysis.

The illustrations are numerous and good and the typography is excellent.

MISCELLANEOUS.

ELECTRIC LIGHT *v.* GAS.—The controversy as to the respective cost of gas and the electric light, if it has not yet been definitely settled, is, we think, on the road towards solution. The figures now vouchsafed to us by the City authorities may, we take it, be relied upon. We learn that at the meeting of the Court of Common Council held last week Mr. Felton, chairman of the Court of Sewers, stated that, of the three electric lighting systems now being tried experimentally in the City, that of the Brush was about the same price as gas, the Lontin was twice the cost, and the Siemens nearly four times the cost. The Brush patentees had, however, given notice to increase their contract price ten per cent., and the Siemens authorities also contemplated increasing their estimate to five times the cost of gas in the same area. As to the new effective gas lighting in Fleet street, he might state that it was over four times the cost of the Brush system in the same district. In reading the above business-like statement, we arrive at two conclusions. In the first place, the effective mode of lighting up the City by arc lamps is nearly as cheap as the indifferent illumination produced by gas. In the second place, it is equally certain that, if gas-light is to be as effective in the open air as the electric light, its cost will be at least three times that of, say, the Brush light. It must, however, be admitted that the latter is the best and cheapest arc light we have at present, its superiority and consequent cheapness being principally due to the excellent quality of the carbons used by the company which owns the light.